LEEDS PHILOSOPHICAL
AND LITERARY SOCIETY

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. LVIII. MARCH 11, 1898. No. 5

SIR R. S. BALL, LL.D., F.R.S., PRESIDENT, in the Chair.

Eric Doolittle, B.Sc., Flower Observatory, University of Pennsylvania, Philadelphia, Pa., U.S.A. ;
Rev. Kingsbury Jameson, M.A., Highfield, Hendon ; and
Alfred Taylor, Polvellan, Holgate Hill, York,

were balloted for and duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

George Banaster, The Mythe Villa, Tewkesbury (proposed by the Rev. C. D. P. Davies) ;
Edward J. Essam, Chemist, Billingborough, Lincolnshire (proposed by the Rev. W. R. Waugh) ;
The Hon. George Stuart Forbes, I.C.S., Revenue Secretary to the Governor of Madras (proposed by C. Michie Smith) ; and
The Rev. William Edward Winks, 58 Richmond Road, Cardiff (proposed by Arthur Mee).

One hundred and seven presents were announced as having been received since the last meeting, including, amongst others :—

Nautical Almanac for 1901, presented by the Lords Commissioners of the Admiralty ; M. W. Meyer, Das Weltgebäude ; and W. Peck, the Observer's Atlas of the Heavens ; presented by the authors.

Note on Dr. Gill's paper "On the Effect of Chromatic Dispersion of the Atmosphere on the Parallaxes of α Centauri and β Orionis, etc." By Arthur A. Rambaut, M.A., Sc.D., Radcliffe Observer.

In the *Monthly Notices* for 1897 December Dr. Gill publishes a paper under the above title, in which he criticises very adversely some conclusions on the same subject which I communicated to the Society in 1895 January.

Before proceeding to examine his arguments I should like to correct an impression which his paper is likely to convey, and to state that the object of my former communication was, not to criticise Dr. Gill's method of discussing his heliometer measures of *α Centauri*, nor to obtain the most probable value of the parallax of this star, but merely to see how far his observations supported the hypothesis that the effect of a difference in the mean refrangibility of the light of two stars is really sensible in refined astronomical measures, and it is only incidentally that the question of the exact numerical value of the parallax enters at all. Thus on p. 135 of the *Monthly Notices*, vol. lv., in the paper referred to, I say : "I have accordingly examined this series of measures, and the three which follow it in the same Memoir, with a view to establishing the existence of such a difference." Again, on p. 141 I say : "My object at present is, not to improve on the value of the parallax deduced . . . but to examine the effect which atmospheric dispersion may have on such measures." It is only at the very end of the paper, in summing up the results of my investigation, that I refer to the values of the parallax which I arrived at in these terms : "Whatever value may be assigned to the particular solutions which I have found, . . . and whether it be thought that the correction I have introduced accounts fully for the systematic error depending on hour angle which runs through them all, and that the still outstanding quantities should be attributed to the accidental errors inseparable from observations of such delicacy and difficulty . . . I think I have shown at least that in parallax measures a term ought to be introduced into the equations of condition of the form

$$d\beta \tan \zeta \cos (p - q)$$

where results of the highest precision are aimed at"—a conclusion which I still think is justified.

Although, however, in my paper above referred to I did not discuss Dr. Gill's method of solving his equations of condition, it has always seemed to me most unsatisfactory to assume empirical corrections to remove the larger residuals and then, appealing to the smallness of the resulting residuals as a proof of the accuracy of the observations, to compute the probable error of the result by means of these reduced residuals.

This method, while it *may* lead to approximate values of the quantities sought, is sure to give a false impression as to the accuracy of the result.

I did not, however, feel that I was in any way called upon to criticise Dr. Gill's solution, nor to express my opinion as to the admissibility of his methods, and therefore I did not at the time examine the point further.

But now that Dr. Gill has attacked my conclusions and condemned my results, the question assumes quite a different complexion. I have no choice but to take up the gage which he has thrown down, and to point out what I believe to be an error in his mode of treating the observations, and especially in estimating the precision of his results.

It is, I think, a mistake in principle to assume the existence of constant errors unless one is absolutely driven to it. The fact of an error of the same sign affecting a number of observations in a series does not prove it to be constant, and in assuming it to be so we assume not only its magnitude but the law of its operation. It is quite different when we suppose the existence of some effect, such as chromatic dispersion, the law of whose operation can be determined *à priori*, and in which we are only in doubt as to the magnitude of the constant factor. In this case, if we force the value of the constant to suit one group of equations, its unreal character will be revealed by the large discrepancies in other groups where its factor is different.

Thus the fact which Dr. Gill quotes against me—namely, that the omission of the terms π and $d\beta$ in the case of β *Orionis* reduces the probable error—seems to show that there is no sensible effect of dispersion or parallax in this series of measures, but has no application to the case of isolated corrections introduced into groups of equations such as Dr. Gill has used.

But in any case, if we assume the existence of constant but unknown errors, and apply corrections for them, we must not ignore the fact that they are *errors*, and we must therefore include them in computing the probable error of an observation. This Dr. Gill has not done, and the computed probable error of his result is consequently very much underestimated. In his recent paper Dr. Gill argues with regard to these corrections that they represent real quantities, with the implication that if this is the case they need not be taken into account in calculating the probable error. But the irregular errors of observation are just

as *real* as the systematic errors, and until we are assured that the latter are really constant, or until we have established the law of their operation, we must treat them as being subject to the law of frequency of errors, and take them into account in estimating the probable error of the result.

Dr. Gill refers to the seriousness of admitting a chromatic dispersion of the amount I have deduced. It is undoubtedly a serious matter if the refraction constant applicable to two neighbouring stars may differ by one or two-tenths of a second; it can, however, in a case like the present be computed and allowed for; but I cannot see how his alternative suggestion that heliometer measures are affected by unknown errors amounting to several tenths of a second is by any means less disquieting.

Now I am not anxious to establish the exact value of the quantity $d\beta$ at which I arrived, and think it very likely that the other errors affecting these measures led to too large a value; nor do I claim that, if judged by the residuals, my solution is superior to Dr. Gill's. There are unquestionably in this series of observations outstanding errors which cannot be fully accounted for by a chromatic dispersion. But

1. I question the validity of Dr. Gill's method of introducing constant corrections to smooth down the roughness of the observations, unless he takes into account the hypothetical character of these corrections in estimating the probable error of the result.

2. I contend that a comparison of the residuals obtained in a direct solution, such as mine, with the empirically corrected residuals which he finds, is no fair test of the relative value of the two solutions.

3. I maintain that the comparison of the mean of the residuals for each group as deduced by him and by me (which he gives on p. 62) is equally fallacious, and that (if it were not for the unavoidable error in the last figure) his means would, from the nature of his solution, be zero in each case.

4. With regard to his statement on p. 57, that "the introduction of terms which have no real values must tend to increase the resulting probable error," I assert that every additional term introduced with a factor unity or (what amounts to the same thing) which is multiplied by the same factor in each equation in which it occurs, must in the solution by least squares reduce the sum of squares of residuals, and that generally, if not always, such terms will have the effect of reducing the probable error whether the real errors be constant or not.

This last point is, indeed, sufficiently obvious; for if in the solution of a series of equations of the form

$$x + by + cz + n = 0$$

we find any considerable number of the residuals grouping themselves around a mean figure, we can at once subtract this mean from each of them and take it across to the other side of the

equation, where in connection with x it will form a new quantity x_1. Indeed, if this principle of correcting equations is admitted without limitations, one can easily get rid of the residuals altogether. Dr. Gill has assumed four such corrections; if he will allow me eight, I can reduce the residuals so that none of them will exceed $0^R{\cdot}010$; twelve such corrections will reduce them all below $0^R{\cdot}005$; while fifty-six would enable me to satisfy the ninety-five equations exactly to three decimal places.

I do not for a moment mean to imply that a solution of this sort is comparable with Dr. Gill's, where his groups were arranged for him by considerations as to the position of the observer's head with regard to the line joining the stars. I merely intend to point out how misleading an appeal to the smallness of the residuals is in such a solution.

In order to see how a solution of this sort would compare with Dr. Gill's, I have solved the equations in the above form, and obtained the residuals given in the fourth column of Table A under the heading "General Solution." They are broken up into four groups as in Dr. Gill's paper, and present all the appearance of being affected by systematic errors. This appearance is, however, very easy to remove, for, taking the mean of the residuals for each group, and applying it with its sign changed as a correction to each, I find the residuals given in the fifth column. These are very similar in character to those found by Dr. Gill; the mean error is not quite so small as Dr. Gill's, being 0·012 as compared with his 0·011. Will Dr. Gill allow that this solution in which the value of π comes out

$$0''{\cdot}819$$

is superior to that I was led to in my paper of 1895, in which the residuals are very much larger?

Hence it is clear that a mere appeal to the residuals is not a safe criterion of the value of a solution. We must consider how those residuals were obtained.

TABLE A.

Group I.

No.	Weight.	Gill.	General Solution.	General Solution corrected.	Separate Solutions.
		R	R	R	R
22	$\frac{1}{2}$	−0·028	−0·007	−0·021	−0·015
23	$\frac{1}{2}$	− 19	0	− 14	− 8
24	...	− 22	− 4	− 18	− 12
25	...	− 7	+ 10	− 4	+ 1
26	...	− 16	0	− 14	− 9
27	...	− 15	0	− 14	− 10
30	...	+ 6	+ 18	+ 4	+ 8

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No.	Weight.	Gill.	General Solution.	General Solution corrected.	Separate Solutions.
		R	R	R	R
32	...	+0·007	+0·017	+0·003	+0·008
33	...	− 11	0	− 14	− 11
34	...	− 12	− 2	− 16	− 13
35	...	+ 1	+ 11	− 3	+ 1
37	...	+ 4	+ 14	0	+ 2
39	½	+ 2	+ 12	− 2	0
42	...	− 5	+ 5	− 9	− 6
45	...	+ 23	+ 34	+ 20	+ 22
46	...	+ 19	+ 29	+ 15	+ 18
47	...	− 1	+ 11	− 3	− 2
50	...	− 7	+ 5	− 9	− 8
52	...	− 6	+ 6	− 8	− 7
56*a*	...	+ 14	+ 31	+ 17	+ 15
61	...	+ 13	+ 33	+ 19	+ 17
63	...	− 1	+ 19	+ 5	+ 2
64	½	+ 12	+ 32	+ 18	+ 15
88	½	− 6	+ 9	− 5	− 20
91	...	+ 7	+ 24	+ 10	− 8
98	...	+ 13	+ 32	+ 18	− 1
104	...	+ 15	+ 39	+ 25	+ 4
		Group II.			
31	...	−0·007	−0·014	−0·012	−0·004
36	...	− 8	− 16	− 14	− 4
38	...	+ 9	+ 1	+ 3	+ 13
40	...	− 11	− 19	− 17	− 6
43	...	+ 3	− 10	− 8	+ 2
54	...	+ 5	+ 1	+ 3	+ 4
55	½	+ 14	+ 11	+ 13	+ 13
59	...	− 1	− 1	+ 1	− 8
60	...	+ 14	+ 15	+ 17	+ 6
90	...	+ 3	+ 2	+ 4	+ 7
95	...	− 5	− 5	− 3	− 4
97	½	− 52	− 51	− 49	− 51
101	...	+ 14	+ 18	+ 20	+ 9
103	...	+ 13	+ 19	+ 21	+ 7

Group III.

No.	Weight.	Gill.	General Solution.	General Solution corrected.	Separate Solutions.
		R	R	R	R
1	...	+0·016	−0·002	+0·013	+0·010
2	...	+ 5	− 12	+ 3	− 1
3	...	− 12	− 29	− 14	− 18
4	...	+ 4	− 13	+ 2	− 2
5	...	0	− 16	− 1	− 5
6	...	− 4	− 20	− 5	− 10
7	...	+ 10	− 5	+ 10	+ 5
9	...	− 2	− 13	+ 2	− 6
10	½	+ 8	− 3	+ 12	+ 5
11	...	+ 9	− 2	+ 13	+ 6
12	...	+ 9	− 1	+ 14	+ 6
13	...	+ 19	+ 9	+ 24	+ 16
14	...	− 16	− 27	− 12	− 20
44	...	+ 13	− 15	0	+ 9
49	...	+ 18	− 9	+ 6	+ 15
51	...	0	− 27	− 12	− 3
53	...	+ 2	− 25	− 10	− 2
58	...	+ 13	− 8	+ 7	+ 12
62	...	− 15	− 36	− 21	− 17
67	...	+ 5	− 9	+ 6	+ 6
68	...	− 6	− 21	− 6	− 5
70	...	+ 16	+ 3	+ 18	+ 18
71	...	− 2	− 15	0	− 1
73	½	+ 4	− 7	+ 8	+ 6
74	½	− 35	− 43	− 28	− 31
76	½	+ 4	− 3	+ 12	+ 8
78	½	+ 2	− 4	+ 11	+ 6
79	...	+ 2	− 3	+ 12	+ 7
80	...	− 3	− 9	+ 6	+ 1
81	½	− 22	− 28	− 13	− 18
82	...	+ 4	− 1	+ 14	+ 9
83	...	− 14	− 19	− 4	− 9
84	...	+ 2	− 3	+ 12	+ 7
92	...	− 13	− 35	− 20	− 8
93	½	− 32	− 53	− 38	− 28
94	...	+ 3	− 18	− 3	+ 8

No.	Weight.	Gill.	General Solution.	General Solution corrected.	Separate Solutions.
		R	R	R	R
96	½	−0·008	−0·028	−0·013	−0·003
99	...	− 4	− 23	− 8	+ 2
100	...	− 2	− 20	− 5	+ 4
102	...	− 25	− 41	− 26	− 18
105	...	+ 9	− 6	+ 9	+ 16
		Group IV.			
17	...	+0·014	+0·033	+0·015	+0·020
18	...	+ 9	+ 28	+ 10	+ 15
19	...	− 15	+ 4	− 14	− 9
20	...	− 38	− 19	− 37	− 32
21	...	+ 3	+ 21	+ 3	+ 8
56	...	− 2	+ 6	− 12	− 7
57	...	+ 20	+ 28	+ 10	+ 15
69	...	− 7	+ 10	− 8	− 10
72	...	− 11	+ 7	− 11	− 15
75	½	+ 25	+ 48	+ 30	+ 22
85	½	− 8	+ 18	0	− 12
86	...	− 23	+ 3	− 15	− 27
87	...	+ 39	+ 64	+ 46	+ 34

But though it is thus obvious that a solution very closely resembling Dr. Gill's, as far as the residuals are concerned, may be obtained by carrying the mean value of a group of residuals from one side of the equation to the other, I have yet to prove that this is practically what Dr. Gill has done, and that a solution by least squares with several x's will necessarily amount to this process.

The equations of condition are of the form

$$x + by + cz + n = 0,$$

where x is a correction to the assumed difference of the mean distances between α *Centauri* and the two comparison stars; y is the change in this depending on the time; while z is the effect of parallax. Dr. Gill has introduced a fourth quantity y' depending on the square of the time due to the orbital motion of α *Centauri* as a double star, but its influence is found to be practically insensible in any of the solutions, and as it does not affect the question at issue between us, I have omitted it for the sake of simplicity.

Excepting chromatic dispersion, these are the only effects which we have any *à priori* reasons for considering. If we solve these equations by the method of least squares we get the residuals in

the fourth column of the table which are found to arrange themselves more or less approximately in the four groups which Dr. Gill has selected. If we denote the various quantities by pairs of subscript numbers, the first applying to the group and the second to the position of the equation in the group, we have the following equations of condition and residuals:

$$\left.\begin{aligned} x + b_{11}y + c_{11}z + n_{11} &= v_{11} \\ x + b_{12}y + c_{12}z + n_{12} &= v_{12} \\ &\text{\&c., \&c.} \end{aligned}\right\} \text{Group I.}$$

$$\left.\begin{aligned} x + b_{21}y + c_{21}z + n_{21} &= v_{21} \\ x + b_{22}y + c_{22}z + n_{22} &= v_{22} \\ &\text{\&c., \&c.} \end{aligned}\right\} \text{Group II.}$$

with similar expressions for the equations of Groups III. and IV.

Now, if we subtract the quantity a_1 from both sides of each equation of Group I.; a_2 from both sides of each equation of Group II., and so for the other groups, and if we denote $x - a_1$ by x_1, $x - a_2$ by x_2, $x - a_3$ by x_3, and $x - a_4$ by x_4, we shall have

$$\left.\begin{aligned} x_1 + b_{11}y + c_{11}z + n_{11} &= v_{11} - a_1 = V_{11}, \text{ say} \\ x_1 + b_{12}y + c_{12}z + n_{12} &= v_{12} - a_1 = V_{12} \quad ,, \\ &\text{\&c., \&c.} \end{aligned}\right\} \text{Group I.}$$

$$\left.\begin{aligned} x_2 + b_{21}y + c_{21}z + n_{21} &= v_{21} - a_2 = V_{21}, \text{ say} \\ x_2 + b_{22}y + c_{22}z + n_{22} &= v_{22} - a_2 = V_{22} \quad ,, \\ &\text{\&c., \&c.} \end{aligned}\right\} \text{Group II.}$$

and so on.

Solving these, by the method of least squares, for x_1, x_2, x_3, x_4, y, and z, we have

$$U = V_{11}^2 + V_{12}^2 + V_{13}^2 + \text{\&c.} + V_{21}^2 + V_{22}^2 + \text{\&c.} + V_{31}^2 + \text{\&c.} + \text{\&c.} = \text{a minimum.}$$

Hence the differential coefficient of this expression with regard to each of the quantities x_1, x_2, x_3, x_4, y and z vanishes. But since

$$\frac{dV_{11}}{dx_1} = \frac{dV_{12}}{dx_1} = \frac{dV_{13}}{dx_1} = \text{\&c.} = 1, \quad \frac{dV_{21}}{dx_1} = \frac{dV_{22}}{dx_1} = \frac{dV_{31}}{dx_1} = \text{\&c.} = 0$$

we have

$$\left.\begin{aligned} \frac{dU}{dx_1} &= V_{11} + V_{12} + V_{13} + \text{\&c.} + V_{1n_1} = 0 \\ \frac{dU}{dx_2} &= V_{21} + V_{22} + V_{23} + \text{\&c.} + V_{2n_2} = 0 \\ \frac{dU}{dx_3} &= V_{31} + V_{32} + V_{33} + \text{\&c.} + V_{3n_3} = 0 \end{aligned}\right\} \quad \ldots\ldots \quad (a)$$

and

$$\left.\begin{aligned} \frac{dU}{dx_4} &= V_{41} + V_{42} + V_{43} + \text{\&c.} + V_{4n_4} = 0 \end{aligned}\right\}$$

Hence the sum of the residuals in each group (and, therefore, the mean) vanishes, and thus Dr. Gill's argument founded on a comparison of the means of the residuals in each group is *of absolutely no value whatever*, his solution being artificially constructed to fulfil the condition that these means should each be zero.

Next we find from the equations (*a*) that

$$\alpha_1 = \frac{\Sigma v_1}{n_1}, \quad \alpha_2 = \frac{\Sigma v_2}{n_2}, \quad \alpha_3 = \frac{\Sigma v_3}{n_3} \text{ and } \alpha_4 = \frac{\Sigma v_4}{n_4}.$$

And we have

$$U = \Sigma (v_1 - \alpha_1)^2 + \Sigma (v_2 - \alpha_2)^2 + \Sigma (v_3 - \alpha_3)^2 + \Sigma (v_4 - \alpha_4)^2 = \Sigma(V^2).$$

With the above values of α_1, α_2, α_3 and α_4 this becomes

$$\Sigma V^2 = \Sigma v^2 - n_1\alpha_1^2 - n_2\alpha_2^2 - n_3\alpha_3^2 - n_4\alpha_4^2.$$

Since n_1, n_2, n_3, and n_4 are positive integers, being the number of observations in each group, it follows that ΣV^2 is necessarily less than Σv^2, and that each additional empirical correction, such as α_1, will have the effect of still further reducing the sum of the squares of the residuals.

Suppose that, in general, there are r unknown quantities, and that we divide the equations into s groups, introducing s constant corrections, we shall have s quantities instead of x, so that the total number of our unknowns will be $r + s - 1 = R$. Thus the error of mean square, or mean error, computed from the final residuals is $\sqrt{\frac{\Sigma (V^2)}{N - R}}$, N being the total number of equations. If this is to be less than the mean error computed from the unreduced residuals, then we must have

$$\frac{\Sigma (v^2)}{N - r} - \frac{\Sigma (V^2)}{N - R}$$

a positive quantity.

But this is equal to

$$\frac{\Sigma (v^2)}{N - r} - \frac{\Sigma (v^2) - n_1\alpha_1^2 - n_2\alpha_2^2 - \&c.}{N - r - s + 1}$$

or

$$\frac{s - 1}{N - r - s + 1} \left\{ \frac{n_1\alpha_1^2 + n_2\alpha_2^2 + \&c.}{s - 1} - \frac{\Sigma (v^2)}{N - r} \right\}$$

In order that this should be positive we must have

$$\frac{n_1\alpha_1^2 + n_2\alpha_2^2 + \ldots}{s - 1} \text{ greater than } \frac{\Sigma (v^2)}{N - r}$$

or, extracting the square root of each and multiplying by K(=0·6745), we must have

$$K \sqrt{\frac{n_1\alpha_1^2 + n_2\alpha_2^2 + \ldots}{s - 1}} \text{ greater than } K \sqrt{\frac{\Sigma (v^2)}{N - r}} \quad \ldots . \quad (b)$$

If the final value of x is deduced from the mean of the s separate values, weighting them in proportion to the number of observations on which each depends, these weights are $\frac{s}{N} n_1$, $\frac{s}{N} n_2$, $\frac{s}{N} n_3$, $\frac{s}{N} n_4$. Then a_1, a_2, &c. being the discordances, the probable error of a result of weight unity is equal to

$$K \sqrt{\frac{s}{N} \frac{n_1 a_1^2 + n_2 a_2^2 + \ldots}{s-1}}$$

and the probable error of the final value is

$$K \sqrt{\frac{n_1 a_1^2 + n_2 a_2^2 + \ldots}{N(s-1)}} = C$$

Now $N-r$ is less than $N-1$, therefore

$$\frac{K}{\sqrt{N}} \sqrt{\frac{\Sigma(v^2)}{N-r}} \text{ is greater than } \frac{K}{\sqrt{N}} \sqrt{\frac{\Sigma(v^2)}{N-1}},$$

and therefore, in consequence of (b), still more is

$$C \text{ greater than } \frac{K}{\sqrt{N}} \sqrt{\frac{\Sigma(v^2)}{N-1}}.$$

But if we suppose the residuals v_{11}, v_{12}, &c. to be due to the uncertainty in x alone, and that the values of y and z are absolutely correct, then the quantity

$$\frac{K}{\sqrt{N}} \sqrt{\frac{\Sigma v^2}{N-1}}$$

is the probable error in the resulting value of x as derived from the first direct solution on this latter hypothesis.

Thus we find the remarkable result, that Dr. Gill has reduced the apparent probable error of an observation of weight unity by the device of applying constant corrections in groups, at the expense of throwing on the resulting value of x a larger degree of uncertainty than would have resulted if we had supposed that the original residuals were wholly due to errors in that quantity, the values of y and z being free from error.

In point of fact the quantity

$$K \sqrt{\frac{\Sigma(V^2)}{N-R}}$$

is *not* the probable error of a single observation, *it is only one portion of it, and not even the more important portion;* and when Dr. Gill's probable error is correctly computed the apparent superiority of his solution wholly disappears.

The oversight which Dr. Gill has here committed is, I think, more common than one would at first suppose, and it is, perhaps, to some extent to errors of this sort that we may

attribute the anomaly of observations by different observers or under different conditions leading to results differing by hundredths or even tenths of a second, while the probable error of each is exhibited as a few units in the third place.

In the case before us the error arises from the hypothetical nature of these corrections having been overlooked in computing the probable error of an observation.

In the fourth part of Airy's *Theory of Errors of Observations* he shows how the existence of constant errors affecting groups of observations is to be established, and points out how the probable error is to be deduced on that hypothesis. Airy, it is true, only considers the case of a single variable, but the principles there laid down are easily extended to the case of any number of variables. See also Chauvenet's *Spherical and Practical Astronomy*, vol. ii. p. 508.

On p. 107, having pointed out how the existence of such constant errors is to be established, Airy goes on to say: "We are not justified for each of these limited groups in assuming a value for the constant error, or variable error of the second class, applicable to that group; we must treat it as an uncertain quantity, and ascertain the combination weights and the probable error and theoretical weight of final result under the effects of the errors of the two classes by an operation analogous to those which are applied when the errors are of only one class."

To investigate the most probable values of the unknown quantities and their probable errors, we suppose that the probable error is the same, or is relatively known, for each observation of a group.

If we have reason to suspect the existence of systematic errors affecting groups of observations, the only legitimate method of proceeding seems to be as follows.

Solve each set of equations separately by the method of least squares. In this way we obtain as many separate values of each variable as there are groups, with their relative weights, the probable error of a single observation of weight unity in each group, and the probable error of each value of each variable. Then, if we introduce the hypothesis of a systematic error affecting each group of observations, we examine by the theory of errors in the way Airy has shown for a single variable, whether such a systematic error is admissible or not. If the result of the examination is in favour of this hypothesis, we investigate the amount of the constant error, or "variable error of the second class," as Airy calls it, for each group, and deduce the most probable value of each variable under the influence of both classes of errors, and the probable error of each final result.

Dr. Gill's method of applying a constant correction in each group, and solving by the method of least squares applied to all the equations of condition thus obtained is quite inadmissible, as may be seen from the following considerations.

Let us suppose we have a series of equations of condition free

from systematic errors, and affected only by the ordinary irregular errors to which the method of Gauss is strictly applicable.

Then, using the same notation as on p. 263, we have :

$$\left.\begin{array}{l} x + b_{11}y + c_{11}z + n_{11} = 0 \\ x + b_{12}y + c_{12}z + n_{12} = 0 \\ \quad \&c. \quad \&c. \\ x + b_{21}y + c_{21}z + n_{21} = 0 \\ \quad \&c. \quad \&c. \end{array}\right\} \quad \ldots\ldots\ldots \quad (A)$$

We have the normal equations

$$\begin{array}{l} \mathrm{N} \,.\, x + \Sigma b \,.\, y + \Sigma c \,.\, z + \Sigma n = 0 \\ \Sigma b \,.\, x + \Sigma(b^2) \,.\, y + \Sigma(bc) \,.\, z + \Sigma(bn) = 0 \\ \Sigma c \,.\, x + \Sigma(bc) \,.\, y + \Sigma(c^2) \,.\, z + \Sigma(cn) = 0 \end{array}$$

and the most probable value of z is, using the determinant notation as in Glaisher's paper in the *Monthly Notices*, vol. xxxiv.

$$z = \begin{vmatrix} \mathrm{N}, & \Sigma b, & \Sigma n \\ \Sigma b, & \Sigma(b^2), & \Sigma(bn) \\ \Sigma c, & \Sigma(bc), & \Sigma(cn) \end{vmatrix} \div \begin{vmatrix} \mathrm{N}, & \Sigma b, & \Sigma c \\ \Sigma b, & \Sigma(b^2), & \Sigma(bc) \\ \Sigma c, & \Sigma(bc), & \Sigma(c^2) \end{vmatrix}$$

Now to the first group of equations let us deliberately apply a constant error α_1 ; to the second group an error α_2, and so on.

Then n_{11} will become $n_{11} + \alpha_1 = \mathrm{N}_{11}$, say
n_{12} ,, ,, $n_{12} + \alpha_1 = \mathrm{N}_{12}$, ,,
&c. &c.
n_{21} ,, ,, $n_{21} + \alpha_2 = \mathrm{N}_{21}$,,
and so on.

And the equations of condition, as they are presented to us for solution, become

$$\left.\begin{array}{l} x + b_{11}y + c_{11}z + \mathrm{N}_{11} = 0 \\ x + b_{12}y + c_{12}z + \mathrm{N}_{12} = 0 \\ \quad \&c \\ x + b_{21}y + c_{21}z + \mathrm{N}_{21} = 0 \\ \quad \&c. \end{array}\right\} \quad \ldots\ldots\ldots \quad (B)$$

But the most probable values of the variables are those found from the equations (A) by the method of least squares. These could be obtained from the equations (B) by subtracting α_1 from the first, α_2 from the second, and so for the other groups. We do not,

however, know the values of α_1, α_2, &c. But putting $x_1 = x - \alpha_1$, $x_2 = x - \alpha_2$, &c. the true equations of condition become

$$\left.\begin{aligned} x_1 + b_{11}y + c_{11}z + \mathrm{N}_{11} &= 0 \\ x_1 + b_{12}y + c_{12}z + \mathrm{N}_{12} &= 0 \\ &\&c. \\ x_2 + b_{21}y + c_{21}z + \mathrm{N}_{21} &= 0 \\ &\&c. \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (C)$$

These are the equations of condition as obtained by Dr. Gill, and we have to ascertain whether, treated by Dr. Gill's method, they will lead to the same result for z as the equations (A).

If we have four groups containing l_1, l_2, l_3, and l_4 observations respectively, the normal equations are :—

$$\begin{aligned} l_1x_1 &\quad + \Sigma b_1 \,.\, y + \Sigma c_1 \,.\, z + \Sigma \mathrm{N}_1 = 0 \\ l_2x_2 &\quad + \Sigma b_2 \,.\, y + \Sigma c_2 \,.\, z + \Sigma \mathrm{N}_2 = 0 \\ l_3x_3 &\quad + \Sigma b_3 \,.\, y + \Sigma c_3 \,.\, z + \Sigma \mathrm{N}_3 = 0 \\ l_4x_4 &\quad + \Sigma b_4 \,.\, y + \Sigma c_4 \,.\, z + \Sigma \mathrm{N}_4 = 0 \\ \Sigma b_1 \,.\, x_1 + \Sigma b_2 \,.\, x_2 + \Sigma b_3 \,.\, x_3 + \Sigma b_4 \,.\, x_4 &+ \Sigma(b^2) \,.\, y + \Sigma(bc) \,.\, z + \Sigma(b\mathrm{N}) = 0 \\ \Sigma c_1 \,.\, x_1 + \Sigma c_2 \,.\, x_2 + \Sigma c_3 \,.\, x_3 + \Sigma c_4 \,.\, x_4 &+ \Sigma(bc) \,.\, y + \Sigma(c^2) \,.\, z + \Sigma(c\mathrm{N}) = 0 \end{aligned}$$

And the value of z is

$$z_1 = \frac{\begin{vmatrix} l_1, & 0, & 0, & 0, & \Sigma b_1, & \Sigma \mathrm{N}_1 \\ 0, & l_2, & 0, & 0, & \Sigma b_2, & \Sigma \mathrm{N}_2 \\ 0, & 0, & l_3, & 0, & \Sigma b_3, & \Sigma \mathrm{N}_3 \\ 0, & 0, & 0, & l_4, & \Sigma b_4, & \Sigma \mathrm{N}_4 \\ \Sigma b_1, & \Sigma b_2, & \Sigma b_3, & \Sigma b_4, & \Sigma(b^2), & \Sigma(b\mathrm{N}) \\ \Sigma c_1, & \Sigma c_2, & \Sigma c_3, & \Sigma c_4, & \Sigma(bc), & \Sigma(c\mathrm{N}), \end{vmatrix}}{\begin{vmatrix} l_1, & 0, & 0, & 0, & \Sigma b_1, & \Sigma c_1 \\ 0, & l_2, & 0, & 0, & \Sigma b_2, & \Sigma c_2 \\ 0, & 0, & l_3, & 0, & \Sigma b_3, & \Sigma c_3 \\ 0, & 0, & 0, & l_4, & \Sigma b_4, & \Sigma c_4 \\ \Sigma b_1, & \Sigma b_2, & \Sigma b_3, & \Sigma b_4, & \Sigma(b^2), & \Sigma(bc) \\ \Sigma c_1, & \Sigma c_2, & \Sigma c_3, & \Sigma c_4, & \Sigma(bc), & \Sigma(c^2) \end{vmatrix}}$$

But

$$\begin{aligned} \Sigma \mathrm{N}_1 &= \Sigma n_1 + l_1\alpha_1 \\ \Sigma \mathrm{N}_2 &= \Sigma n_2 + l_2\alpha_2 \\ \Sigma \mathrm{N}_3 &= \Sigma n_3 + l_3\alpha_3 \\ \Sigma \mathrm{N}_4 &= \Sigma n_4 + l_4\alpha_4 \\ \Sigma(b\mathrm{N}) &= \Sigma(bn) + \Sigma b_1 \,.\, \alpha_1 + \Sigma b_2 \,.\, \alpha_2 + \Sigma b_3 \,.\, \alpha_3 + \Sigma b_4 \,.\, \alpha_4 \\ \Sigma(c\mathrm{N}) &= \Sigma(cn) + \Sigma c_1 \,.\, \alpha_1 + \Sigma c_2 \,.\, \alpha_2 + \Sigma c_3 \,.\, \alpha_3 + \Sigma c_4 \,.\, \alpha_4. \end{aligned}$$

Hence multiplying the first four columns by α_1, α_2, α_3, and α_4 respectively, and subtracting from the last, this expression becomes

$$z_1 = \frac{\begin{vmatrix} l_1, & 0, & 0, & 0, & \Sigma b_1 & \Sigma n_1 \\ 0, & l_2, & 0, & 0, & \Sigma b_2, & \Sigma n_2 \\ 0, & 0, & l_3, & 0, & \Sigma b_3, & \Sigma n_3 \\ 0, & 0, & 0, & l_4, & \Sigma b_4, & \Sigma n_4 \\ \Sigma b_1, & \Sigma b_2, & \Sigma b_3, & \Sigma b_4, & \Sigma(b^2), & \Sigma(bn) \\ \Sigma c_1, & \Sigma c_2, & \Sigma c_3, & \Sigma c_4, & \Sigma(bc), & \Sigma(cn) \end{vmatrix}}{\begin{vmatrix} l_1, & 0, & 0, & 0, & \Sigma b_1, & \Sigma c_1 \\ 0, & l_2, & 0, & 0, & \Sigma b_2, & \Sigma c_2 \\ 0, & 0, & l_3, & 0, & \Sigma b_3, & \Sigma c_3 \\ 0, & 0, & 0, & l_4, & \Sigma b_4, & \Sigma c_4 \\ \Sigma b_1, & \Sigma b_2, & \Sigma b_3, & \Sigma b_4, & \Sigma(b^2), & \Sigma(bc) \\ \Sigma c_1, & \Sigma c_2, & \Sigma c_3, & \Sigma c_4, & \Sigma(bc), & \Sigma(c^2) \end{vmatrix}}$$

Expanding these determinants each in terms of the minors of its fifth and sixth rows, this expression becomes after a slight transformation

$$l_1l_2l_3l_4\begin{vmatrix}\Sigma b^2, & \Sigma(bn)\\ \Sigma(bc), & \Sigma(cn)\end{vmatrix}+l_2l_3l_4\begin{vmatrix}0, & \Sigma b_1, & \Sigma n_1\\ \Sigma b_1, & \Sigma(b^2), & \Sigma(bn)\\ \Sigma c_1, & \Sigma(bc), & \Sigma(cn)\end{vmatrix}+l_1l_3l_4\begin{vmatrix}0, & \Sigma b_2, & \Sigma n_2\\ \Sigma b_2, & \Sigma(b^2), & \Sigma(bn)\\ \Sigma c_2, & \Sigma(bc), & \Sigma(cn)\end{vmatrix}$$

$$+l_1l_2l_4\begin{vmatrix}0, & \Sigma b_3, & \Sigma n_3\\ \Sigma b_3, & \Sigma(b^2), & \Sigma(bn)\\ \Sigma c_3, & \Sigma(bc), & \Sigma(cn)\end{vmatrix}+l_1l_2l_3\begin{vmatrix}0, & \Sigma b_4, & \Sigma n_4\\ \Sigma b_4, & \Sigma(b^2), & \Sigma(bn)\\ \Sigma c_4, & \Sigma(bc), & \Sigma(cn)\end{vmatrix}+l_1l_2\begin{vmatrix}0, & 0, & \Sigma b_3, & \Sigma n_3\\ 0, & 0, & \Sigma b_4, & \Sigma n_4\\ \Sigma b_3, & \Sigma b_4, & 0, & 0\\ \Sigma c_3, & \Sigma c_4, & 0, & 0\end{vmatrix}$$

$$+l_1l_3\begin{vmatrix}0, & 0, & \Sigma b_2, & \Sigma n_2\\ 0, & 0, & \Sigma b_4, & \Sigma n_4\\ \Sigma b_2, & \Sigma b_4, & 0, & 0\\ \Sigma c_2, & \Sigma c_4, & 0, & 0\end{vmatrix}+l_1l_4\begin{vmatrix}0, & 0, & \Sigma b_2, & \Sigma n_2\\ 0, & 0, & \Sigma b_3, & \Sigma n_3\\ \Sigma b_2, & \Sigma b_3, & 0, & 0,\\ \Sigma c_2, & \Sigma c_3, & 0, & 0,\end{vmatrix}+l_2l_3\begin{vmatrix}0, & 0, & \Sigma b_1, & \Sigma n_1\\ 0, & 0, & \Sigma b_4, & \Sigma n_4\\ \Sigma b_1, & \Sigma b_4, & 0, & 0\\ \Sigma c_3, & \Sigma c_4, & 0, & 0\end{vmatrix}$$

$$+l_2l_4\begin{vmatrix}0, & 0, & \Sigma b_1, & \Sigma n_1\\ 0, & 0, & \Sigma b_3, & \Sigma n_3\\ \Sigma b_1, & \Sigma b_3, & 0, & 0\\ \Sigma c_1, & \Sigma c_3, & 0, & 0\end{vmatrix}+l_3l_4\begin{vmatrix}0, & 0, & \Sigma b_1, & \Sigma n_1\\ 0, & 0, & \Sigma b_2, & \Sigma n_2\\ \Sigma b_1, & \Sigma b_2, & 0, & 0\\ \Sigma c_1, & \Sigma c_2, & 0, & 0\end{vmatrix}$$

Divided by the expression into which this transforms when c is substituted for n throughout.

But the value of z as derived from the equations (A) is

$$\begin{vmatrix}N, & \Sigma b, & \Sigma n\\ \Sigma b, & \Sigma(b^2), & \Sigma(bn)\\ \Sigma c, & \Sigma(bc), & \Sigma(cn)\end{vmatrix}$$

Divided by the expression into which this transforms when c is substituted for n.

Now, the quantities l_1, l_2, l_3, l_4, and N bear no relation to the coefficients of the normal equations, and we know that if α, β, γ, δ, ϵ and α', β', γ', δ', ϵ' are so related that for any arbitrary multipliers p, q, r, s, t

$$\frac{p\alpha+q\beta+r\gamma}{p\alpha'+q\beta'+r\gamma'}=\frac{s\delta+t\epsilon}{s\delta'+t\epsilon'}$$

then

$$\frac{\alpha}{\alpha'}=\frac{\beta}{\beta'}=\frac{\gamma}{\gamma'}=\frac{\delta}{\delta'}=\frac{\epsilon}{\epsilon'}$$

From this it follows that the two values of z will agree if

$$\frac{n_{11}}{c_{11}}=\frac{n_{12}}{c_{12}}=\frac{n_{13}}{c_{13}}=\&c.=\frac{n_{21}}{c_{21}}=\frac{n_{22}}{c_{22}}=\&c.=\frac{n_{31}}{c_{31}}\ \&c.$$

Apart, then, from a very complicated relation between the numbers of the equations in each group and the coefficients of

the normal equations, which we have no right to assume, this seems to be the only condition of a general character that the method under consideration may be allowable.

It is, however, quite different if we treat each group of equations separately. Then we get no contradiction, but the resulting value of z, as deduced from each group affected with constant error, is the same as if the constant error did not exist. This almost obvious proposition may be easily demonstrated as follows :

If we confine our attention to the first group, and suppose that each equation in this is affected by a systematic error, which for the sake of greater generality we may take to be of the form

$$\alpha_1 + b_{11}\beta_1 ; \ \alpha_1 + b_{12}\beta_1, \ \&c. ;$$

and if we put

$$N_{11} = n_{11} + \alpha_1 + b_{11}\beta_1 ; \ N_{12} = n_{12} + \alpha_1 + b_{12}\beta_1 ; \ \&c.,$$

the equations as presented to us for solution become

$$x + b_{11}y + c_{11}z + N_{11} = 0, \ \&c. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (D)$$

but the equations of condition unaffected by systematic error may be written

$$x + b_{11}y + c_{11}z + N_{11} - \alpha_1 - b_{11}\beta_1 = 0, \ \&c.$$

Hence if we put $x_1 = x - \alpha_1$; $y_1 = y - \beta_1$, we get

$$x_1 + b_{11}y_1 + c_{11}z + N_{11} = 0,$$

which are the true equations of condition, although N_{11}, &c., are affected with systematic errors.

The normal equations thus become

$$\begin{aligned} l_1 x_1 + \Sigma b_1 \ . \ y_1 + \Sigma c_1 \ . \ z + \Sigma N_1 &= 0 \\ \Sigma b_1 \ . \ x_1 + \Sigma(b_1^2) \ . \ y_1 + \Sigma(bc_1) \ . \ z + \Sigma(bN_1) &= 0 \\ \Sigma c_1 \ . \ x_1 + \Sigma(bc_1) \ . \ y_1 + \Sigma(c_1^2) \ . \ z + \Sigma(cN_1) &= 0 \end{aligned}$$

from which we deduce

$$z = \begin{vmatrix} l_1, & \Sigma b_1, & \Sigma N_1 \\ \Sigma b_1, & \Sigma(b_1^2), & \Sigma(bN_1) \\ \Sigma c_1, & \Sigma(bc_1), & \Sigma(cN_1) \end{vmatrix} \div \begin{vmatrix} l_1, & \Sigma b_1, & \Sigma c_1 \\ \Sigma b_1, & \Sigma(b_1^2), & \Sigma(bc_1) \\ \Sigma c_1, & \Sigma(bc_1), & \Sigma(c_1^2) \end{vmatrix}$$

which transforms into

$$z = \begin{vmatrix} l_1, & \Sigma b_1, & \Sigma n_1 \\ \Sigma b_1, & \Sigma(b_1^2), & \Sigma(bn_1) \\ \Sigma c_1, & \Sigma(bc_1), & \Sigma(cn_1) \end{vmatrix} \div \begin{vmatrix} l_1, & \Sigma b_1, & \Sigma c_1 \\ \Sigma b_1, & \Sigma(b_1^2), & \Sigma(bc_1) \\ \Sigma c_1, & \Sigma(bc_1), & \Sigma(c_1^2) \end{vmatrix}$$

which is the same expression as we should have obtained from the original equations of condition.

We thus see that if each group is affected by a constant error, or in addition with an error varying with the time, the factor (β) of which varies from group to group, then by solving each group of equations separately we eliminate entirely the effect of this systematic error from the values of z, and obtain the same values for this quantity as we should have found had we been able to determine this error and eliminate its effect from the equations of condition. If there is a further term in the expression for the error of the form $c_{11}\gamma_1$, $c_{12}\gamma_1$ . . . $c_{21}\gamma_2$, $c_{22}\gamma_2$ . . . then we shall no longer be able to eliminate it in the separate groups; we shall, in fact, not be able to separate γ_1 from z in the first group, γ_2 from z in the second, and so on. We shall thus get as many values of z as there are groups, and the final value of z will be found by combining these in such a way that its probable error may be the least possible.

Should the systematic error not be represented, or not wholly represented, by the expressions $a_1+b_{11}\beta_1+c_{11}\gamma_1$, &c., $a_2+b_{21}\beta_2+c_{21}\gamma_2$, &c., there will still be an outstanding effect which we cannot eliminate, but its trend or law is presumably wholly unknown to us, and its effect will be indistinguishable from the ordinary errors of observation.

We see, therefore, that whatever be the law of systematic error we must solve each group of equations separately, and investigate the weight with which the individual results are to be combined, so as to make the probable error arising from every source a minimum.

In the case of Dr. Gill's equations of condition, the coefficient of x is unity, but for the sake of generality we shall consider a series of equations of the form

$$ax+by+cz+n=0.$$

If we suppose these equations to be divided into groups in each of which every observation is affected by a systematic error, the procedure will be similar whatever be the numbers of the variables or of the groups—we find the following quantities for each group in the ordinary way by the application of the principle of least squares :—

Group.	I.	II.
Probable error of equation of weight = 1	e_1	e_2
Value of x	x_1	x_2
Weight of x	p_{x1}	p_{x2}
Probable error of x	e_{x1}	e_{x2}
Value of y	y_1	y_2
Weight of y	p_{y1}	p_{y2}
Probable error of y	e_{y1}	e_{y2}
Value of z	z_1	z_2
Weight of z	p_{z1}	p_{z2}
Probable error of z	e_{z1}	e_{z2} &c. &c.

Now, to decide whether the separate values of any of these variables are affected by systematic errors we proceed as Airy directs in the case of a single variable.

The next few paragraphs are, indeed, almost word for word taken from his *Theory of Errors of Observations*.

We take the weighted mean of the separate values, the weights being inversely proportional to the squares of their probable errors. This mean may be provisionally regarded as "final result." The "Final Result" is to be subtracted from the "Result for each Group," and the remainder is the "Discordance for each Group."

Now we treat the discordances as being themselves errors, and investigate by the theory of errors the "Mean Discordance;" and the "Probable Error of Mean Discordance" is then found in the way that Airy indicates.

Thus we have (1) a numerical value of mean discordance, and (2) a number expressing the probable error in the determination of that numerical value.

If (2) exceeds (1) there is no justification for the assumption of a discordance, that is, of a systematic error. If (2) is much less than (1) it appears equally clear that a systematic error must be assumed to exist, and (1) or any value near it may be taken for "mean discordance." The "*probable* discordance" or "*probable* systematic error" will then be found by multiplying this by 0·8454.

Now we are not justified in assuming that the "discordance for each group," found above, is *actually* the systematic error or "variable error of the second class" applicable to that group; we must treat these errors as uncertain quantities and ascertain the combination weights, and the probable errors and theoretical weights of the final results under the effects of the errors of the two classes.

Let the *actual* values of the errors of the second class affecting one of the variables, say x, in the successive groups be C_1, C_2, C_3, C_4; the probable error of each being c. And in the first group let the *actual* values of the errors of the first class (or ordinary errors) affecting successive observations be E_{11}, E_{12}, E_{13}, &c.; for those in the second group E_{21}, E_{22}, &c., and so on. Their probable values being e_{11}, e_{12}, e_{13}, &c., e_{21}, e_{22}, &c.

The actual error of the first class in one of the separate values of the variable, say the first, consists of a series of multiples of the quantities E_{11}, E_{12}, &c.; hence we may write the total *actual* error affecting x_1 as

$$C_1 + a_{11}E_{11} + a_{12}E_{12} + a_{13}E_{13} + \&c.$$

In order to find a final value of x we assume four undetermined multipliers, l_1, l_2, l_3, and l_4, and take

$$x = \frac{l_1x_1 + l_2x_2 + l_3x_3 + l_4x_4}{l_1 + l_2 + l_3 + l_4} = \frac{\Sigma lx}{\Sigma l},$$

and the most probable value of x is that computed with the quantities l_1, l_2, l_3, l_4, for which the resulting probable error of x is a minimum.

To find these quantities we notice that the actual error in x is

$$\frac{l_1C_1 + l_2C_2 + l_3C_3 + l_4C_4}{\Sigma l} + \frac{l_1}{\Sigma l}(a_{11}E_{11} + a_{12}E_{12} + \&c.) + \frac{l_2}{\Sigma l}(a_{21}E_{21} + a_{22}E_{22} + \&c.) + \&c.$$

Hence the square of the probable error in x is

$$\frac{l_1^2 + l_2^2 + l_3^2 + l_4^2}{(\Sigma l)^2}c^2 + \frac{l_1^2}{(\Sigma l)^2}\{a_{11}^2e_{11}^2 + a_{12}^2e_{12}^2 + a_{13}^2e_{13}^2 + \&c.\} + \frac{l_2^2}{(\Sigma l)^2}\{a_{21}^2e_{21}^2 + \&c.\} + \&c.$$

But $a_{11}^2 e^2_{11} + a_{12}^2 e_{12}^2 + \&c.$ is the square of the probable error of x, as deduced from the first group of observations alone; $a_{21}^2 e_{21}^2 + a_{22}^2 e_{22}^2 + \&c.$ is the square of the probable error of x, as deduced from the second group of observations alone, and so on. But these are equal to $\frac{e_1^2}{p_{x1}}$, $\frac{e_2^2}{p_{x2}}$, and so on.

Hence, we find the square of the probable error of x is

$$\frac{l_1^2 + l_2^2 + l_3^2 + l_4^2}{(\Sigma l)^2} \cdot c^2 + \frac{l_1^2}{(\Sigma l)^2} \cdot \frac{e_1^2}{p_{x1}} + \frac{l_2^2}{(\Sigma l)^2} \cdot \frac{e_2^2}{p_{x2}} + \&c.$$

and this is to be a minimum.

Differentiating with respect to each of the quantities l_1, l_2, l_3 and l_4, and putting the differential coefficients equal to zero we find, putting A for an indeterminate constant,

$$l_1c^2 + l_1\frac{e_1^2}{p_{x1}} = A.$$

Therefore,

$$l_1 = \frac{A}{c^2 + \frac{e_1^2}{p_{x1}}}$$

similarly

$$l_2 = \frac{A}{c^2 + \frac{e_2^2}{p_{x2}}}$$

with similar values for l_3 and l_4.

These are the combination factors for each of the separate results. If c is zero, they reduce to the ordinary form, viz., $\frac{p_{x1}}{e_1^2}$, $\frac{p_{x2}}{e_2^2}$, &c., or to the reciprocals of the squares of the probable errors of the individual results.

The square of the probable error of x found above may be written

$$\frac{l_1^2}{(\Sigma l)^2}\left\{c^2 + \frac{e_1^2}{p_{x1}}\right\} + \frac{l_2^2}{(\Sigma l)^2}\left\{c^2 + \frac{e_2^2}{p_{x2}}\right\} + \frac{l_3^2}{(\Sigma l)^2}\left\{c^2 + \frac{e_3^2}{p_{x3}}\right\} + \frac{l_4^2}{(\Sigma l)^2}\left\{c^2 + \frac{e_4^2}{p_{x4}}\right\}$$

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or, if we substitute the values found above for l_1, l_2, l_3 and l_4,

$$\frac{1}{e_x^2} = \frac{1}{c^2 + \frac{e_1^2}{p_{x1}}} + \frac{1}{c^2 + \frac{e_2^2}{p_{x2}}} + \frac{1}{c^2 + \frac{e_3^2}{p_{x3}}} + \frac{1}{c^2 + \frac{e_4^2}{p_{x4}}},$$

where e_x denotes the probable error of the final value of x.

From the method of deduction it is obvious that we have similar expressions for y and z, viz. :

$$\frac{1}{e_y^2} = \frac{1}{d^2 + \frac{e_1^2}{p_{y1}}} + \frac{1}{d^2 + \frac{e_2^2}{p_{y2}}} + \frac{1}{d^2 + \frac{e_3^2}{p_{y3}}} + \frac{1}{d^2 + \frac{e_4^2}{p_{y4}}},$$

and

$$\frac{1}{e_z^2} = \frac{1}{f^2 + \frac{e_1^2}{p_{z1}}} + \frac{1}{f^2 + \frac{e_2^2}{p_{z2}}} + \frac{1}{f^2 + \frac{e_3^2}{p_{z3}}} + \frac{1}{f^2 + \frac{e_4^2}{p_{z4}}},$$

in which d and f are the probable constant errors affecting the separate values of y and z. The combination factors are the separate fractions in these expressions.

We thus see that, in order to combine the results for any physical quantity obtained from different groups of observations which may possibly be affected by discordances which are constant in each group, we do not require to know the original equations from which each separate result is obtained in order to deduce the most probable result. The only quantities we need are the most probable results as deduced from each series by the method of least squares, the probable error of an observation of weight unity in each series, and the weight of the resulting values relatively to that of an observation of weight unity.

We have now to solve the equations of condition deduced from Dr. Gill's observations in accordance with the theory above developed.

First we solve the four groups of equations separately by the method of least squares, and we find the following results :—

TABLE B.

Group.	I.	II.	III.	IV.
	R	R	R	R
e	± 0·0072	± 0·0090	± 0·0095	± 0·0144
	R	R	R	R
x	+ 0·0075	+ 0·0126	− 0·0182	+ 0·0089
p_x	4·657	1·001	20·587	4 280
	R	R	R	R
e_x	± 0·0033	± 0 0090	± 0 0021	± 0·0070
	R	R	R	R
y	+ 0·0108	− 0·0030	− 0·0107	+ 0·0083
p_y	4·215	3·018	10·724	1·844
	R	R	R	R
e_y	± 0·0035	± 0·0052	± 0·0029	± 0·0106
	R	R	R	R
z	+ 0·1181	+ 0·0912	+ 0·1118	+ 0·1129
p_z	2·780	0·850	13·824	3·006
	R	R	R	R
e_z	± 0·0043	± 0·0098	± 0·0026	± 0·0083

Let us first consider the values of x. If we give each of the four values a combination-weight inversely proportional to the square of its probable error, we find as the mean value

$$x = -0{\cdot}0089.$$

Hence we have as the discordances from the mean

$$\Delta_1 = +0{\cdot}0164,$$
$$\Delta_2 = +0{\cdot}0215,$$
$$\Delta_3 = -0{\cdot}0093,$$
$$\Delta_4 = +0{\cdot}0178,$$

and the mean discordance, as defined by Airy, p. 19, is equal to

$$\Delta = 0{\cdot}0139.$$

Now the actual error of the first class in x_1 being E_1; of x_2 being E_2, &c.; the actual error of x will be $\frac{\Sigma(p_x \times E)}{\Sigma(p_x)}$. Hence, the actual error of Δ_1 is $E_1 - \frac{\Sigma(p_x \times E)}{E(p_x)}$; of Δ_2 is $E_2 - \frac{\Sigma(p_x E)}{\Sigma(p_x)}$, &c. In this case, $\Delta = \frac{1}{6}(\Delta_1 + \Delta_2 + \Delta_4) - \frac{1}{2}\Delta_3$, and therefore the actual error in Δ is

$$\tfrac{1}{6}(E_1 + E_2 + E_4) - \tfrac{1}{2}E_3,$$

and consequently, if e_Δ denotes the probable error of Δ, we have

$$e_\Delta{}^2 = \tfrac{1}{36}(e_{x_1}{}^2 + e_{x_2}{}^2 + e_{x_4}{}^2) + \tfrac{1}{4}e_{x_3}{}^2.$$

[It might perhaps be better to take the mean discordance as defined by Chauvenet, viz., as the mean of the discordances without regard to sign. This would give us in this case $\Delta = 0{\cdot}0162$, and in all cases the probable error of Δ would then be—

$$e_\Delta = \pm \frac{\sqrt{e_{x_1}{}^2 + e_{x_2}{}^2 + e_{x_3}{}^2 + \&c.}}{s}.]$$

Putting in the values of e_{x_1}, &c. from *Table B*, and extracting the square root, we find $e_\Delta = \pm 0{\cdot}0022$. Hence, we have (1) the "Mean Discordance" equal to 0·0139, and (2), its probable error equal to $\pm 0{\cdot}0022$. In this case the probable error of Δ is so much smaller than Δ itself that we are probably justified in assuming the reality of Δ.

We thus find for the "Mean Discordance," or "Mean Error of the Second Class," the quantity 0·0139, which multiplied by the factor 0·8454 gives us as the "Probable Error of the Second Class"

$$c = \pm 0{\cdot}0117.$$

We have next to compute the combination factors $1/(c^2+e_{x_1}{}^2)$, &c. Thus we find—

$$1/(c^2+e_{x_1}{}^2) = 6756$$
$$1/(c^2+e_{x_2}{}^2) = 4590$$
$$1/(c^2+e_{x_3}{}^2) = 7079$$

and

$$1/(c^2+e_{x_4}{}^2) = 5395$$
$$\therefore 1/e_x{}^2 = 23820 \quad \therefore e_x = \pm 0^{\mathrm{R}}\cdot 0065$$

And therefore $x = +0^{\mathrm{R}}\cdot 0012 \pm 0^{\mathrm{R}}\cdot 0065 = +0''\cdot 015 \pm 0''\cdot 084$.

Again let us examine the most probable value of z. Proceeding as in the case of x, we find

$$z = +0^{\mathrm{R}}\cdot 1124,$$

and for the discordances from this mean we have

$$\Delta_1 = -0^{\mathrm{R}}\cdot 0057$$
$$\Delta_2 = +0^{\mathrm{R}}\cdot 0212$$
$$\Delta_3 = +0^{\mathrm{R}}\cdot 0006$$
$$\Delta_4 = -0^{\mathrm{R}}\cdot 0005$$

and the mean discordance is equal to $0^{\mathrm{R}}\cdot 0070$.

In this case $\Delta = \frac{1}{4}(\Delta_2+\Delta_3-\Delta_1-\Delta_4)$, and therefore we find that

$$e_\Delta{}^2 = \tfrac{1}{16}(e_{z_1}{}^2+e_{z_2}{}^2+e_{z_3}{}^2+e_{z_4}{}^2)$$

Substituting the values of e_{z_1}, &c. from *Table B*, and extracting the square root, we have $e_\Delta = \pm 0^{\mathrm{R}}\cdot 0034$.

Hence we have for the "Mean Error of the Second Class" in z

$$\Delta = 0^{\mathrm{R}}\cdot 0070 \pm 0^{\mathrm{R}}\cdot 0034.$$

The magnitude of the probable error here, being just less than half that of Δ itself, would seem to point to the existence of systematic errors in the values of z, though not so convincingly as in the case of x. If we admit its existence, we have as the "Probable Error of the Second Class" in z

$$f = \pm 0^{\mathrm{R}}\cdot 0059.$$

Assuming this value for f, we have next to compute the combination factors as before, and we find—

$$1/(f^2+e_{z_1}{}^2) = 18706$$
$$1/(f^2+e_{z_2}{}^2) = 7683$$
$$1/(f^2+e_{z_3}{}^2) = 24190$$
$$1/(f^2+e_{z_4}{}^2) = 9635$$
$$1/e_z{}^2 = 60214 \quad \therefore e_z = \pm 0^{\mathrm{R}}\cdot 0041,$$

and therefore—

$$z = +0^{\mathrm{R}}\cdot 1113 \pm 0^{\mathrm{R}}\cdot 0041 = 0''\cdot 761 \pm 0''\cdot 028.$$

If we do not admit the existence of a systematic error in the values of z, we must put $f=0$,* and we get simply—

$$\frac{1}{e_z^2}=\frac{1}{e_{z_1}^2}+\frac{1}{e_{z_2}^2}+\frac{1}{e_{z_3}^2}+\frac{1}{e_{z_4}^2}=226941$$

$$\therefore \frac{1}{e_z}=\pm 476{\cdot}4$$

and

$$\therefore e_z=\pm 0{\cdot}0021.$$

We thus get the two solutions—

(1) Assuming the existence of systematic errors affecting the values of z in each group,

$$\pi=0{\cdot}1113\pm 0{\cdot}0041=0{\cdot}761\pm 0{\cdot}028,$$

or (2) supposing that there is no such systematic error—

$$\pi=0{\cdot}1124\pm 0{\cdot}0021=0{\cdot}769\pm 0{\cdot}014.$$

And now let us turn once more to Dr. Gill's solution. It is not very clear what the probable error of an observation really is. It is certainly not permissible to postulate the existence of unknown errors in the equations, and then to ignore them in computing the probable error. If we admit them at all we must consider them as uncertain errors, and treat them as such.

Dr. Gill supposes a constant error α_1 to exist in each equation of the first group; a constant error α_2 in each equation of the second group, and so on, which, in combination with x, give him four quantities—

$$x_{,}=x+\alpha_1;\ x_{,,}=x+\alpha_2;\ x_{,,,}=x+\alpha_3;\ \text{and}\ x_{,,,,}=x+\alpha_4.$$

Now, if we treat these as we did the four values of x in the above separate solutions, we shall be able to obtain a "Mean Discordance" and a "Probable Constant Error" as before. Combining the quantities $x_{,}$, $x_{,,}$, &c., with the weights as given by Dr. Gill, a mean value of x is obtained, which we subtract from each of the individual values, and thus obtain a discordance for each group. From these we form the "Mean Discordance," and then, multiplying by 0·8454, we get the "Probable Constant Error."

* It is not, however, by any means clear that we are at liberty to put $f=0$, and I am inclined to think that, having admitted the existence of systematic errors affecting one of the variables, we must treat the discordances in the separate values of the others as being due to the same cause.

Thus

$x_{,} = +0^{R}\cdot0178$	Weight 17·2	$\Delta_1 = +0^{R}\cdot0199$
$x_{,,} = -0^{R}\cdot0003$	,, 9·5	$\Delta_2 = +0^{R}\cdot0018$
$x_{,,,} = -0^{R}\cdot0214$	,, 24·7	$\Delta_3 = -0^{R}\cdot0193$
$x_{,,,,} = +0^{R}\cdot0093$	,, 9·7	$\Delta_4 = +0^{R}\cdot0114$
	61·1	$\Delta = 0^{R}\cdot0152$

Hence $x = -0^{R}\cdot0021$. Subtracting this from $x_{,}$, $x_{,,}$, &c., we find Δ_1 Δ_2, &c., given in the last column, and thus we have—

$$\Delta = 0^{R}\cdot0152.$$

Therefore the "Probable Constant Error" is $\pm 0^{R}\cdot0128$. This is the uncertainty by which the observations are affected in virtue of the constant (but unknown) errors attaching to the separate groups.

Now if E_{11}, E_{12}, &c., E_{21}, E_{22}, &c., are the errors of the ordinary kind attaching to the observations, we have the total errors of the separate observations in the first group—

$$\alpha_1 + E_{11},\ \alpha_1 + E_{12},\ \alpha_1 + E_{13},\ \&c.;$$

in the second group—

$$\alpha_2 + E_{21},\ \alpha_2 + E_{22},\ \alpha_2 + E_{3},\ \&c.$$

and so on.

If we suppose the probable value of each of the quantities E_{11} E_{12}, &c., E_{21} E_{22}, &c., to be e, and denote the probable constant error by α, it would seem that the square of the probable error of an observation under the effect of both classes of error is equal to

$$\alpha^2 + e^2.$$

But $\alpha = \pm 0^{R}\cdot0128$.

And if for e we take the quantity given by Dr. Gill, namely, $\pm 0^{R}\cdot0091$8, we find the probable error of an observation deduced in this way equal to

$$\pm 0^{R}\cdot0158,$$

and, therefore, the probable error of z, if deduced in the ordinary way, would be

$$\pm 0^{R}\cdot0033.$$

Hence, we have finally

$$\pi = 0''\cdot747 \pm 0''\cdot023.$$

It appears, therefore, that the probable error as given by Dr. Gill (viz., $\pm 0''\cdot013$) should be increased in the proportion of 13

to 23 ; or that Dr. Gill has overestimated the weight of his results in the proportion of

$$(23)^2 : (13)^2$$

or approximately as 3 : 1.

But, indeed, the term "probable error" cannot properly be applied to the quantity $\pm 0''{\cdot}023$ found above ; still less is it applicable to the quantity $\pm 0''{\cdot}013$ found by Dr. Gill ; for the value $0''{\cdot}747$ is not deduced in accordance with the theory of errors, and we have no guarantee that the value $e = 0{\cdot}0092$ which we have adopted from Dr. Gill represents the probable error of the first class as deduced from the equations.

As Airy says, in another connection, on the last page of his *Theory of Errors of Observations*, "The computer apparently has had his attention engrossed by 'minimum squares' as the important result to be attained, whereas in reality the satisfying of the equations for minimum squares produces a merely accidental coincidence of results in certain cases (not in all) with those leading to the 'minimum probable error of final result' which is the legitimate object of research."

To recapitulate, then,

(1) If we take the equations as they stand and solve directly, we find

$$\pi = 0''{\cdot}819 \pm 0''{\cdot}014.$$

Judged by their probable errors alone this and the next one appear the best results of all. The residuals from this solution, however, show it to be inadmissible, and the deduced value of the parallax is probably much too large.

(2) If we admit the possibility of constant errors in each group, and solve by the theory of errors, we have the result

$$\pi = 0''{\cdot}769 \pm 0''\ 014$$

while the most probable values of the constant errors are not those found by Dr. Gill.

(3) If we admit in addition the possibility of analogous errors affecting the values of π as found from the various groups (for which we have some evidence, although it is not so convincing as in the case of x), we find

$$\pi = 0''{\cdot}761 \pm 0''{\cdot}028.$$

(4) We have the solution given by me in my former paper, in which the introduction of a term representing the corrections for chromatic dispersion in the atmosphere leads to the value

$$\pi = 0''{\cdot}780 \pm 0''{\cdot}018$$

and (5) we have Dr. Gill's solution by a method for which there does not seem to be any logical sanction. When we take into

account the fallibility of the corrections which he introduces, this gives us

$$\pi = 0''\cdot747$$

with a probable error of $\pm 0''\cdot023$, as far as it is possible to estimate the probable error at all.

The principal results of this examination are, therefore—

(1) That there appear to be errors affecting each group which are *not* those found by Dr. Gill, and that the evidence is by no means conclusive that they are constant in each group, but that, on the contrary, there is a presumption in favour of a systematic error affecting the separate values of the parallax as deduced from the various groups.

(2) That Dr. Gill's method is inadmissible and his probable error underestimated.

(3) That the most probable value of the parallax as deduced from this series of measures on the assumption of constant errors seems to be $0''\cdot77$, and that the introduction of the correction for chromatic dispersion reduces the value of π from $0''\cdot82$ given by the general solution to $0''\cdot78$ or to within $0''\cdot01$ of this value.

And (4) That the value of π found by Dr. Gill, viz.—$0''\cdot747$, cannot by any logical process be deduced from this series of observations.

The Effect of Latitude Variation on the Ecliptic Investigation. By W. G. Thackeray.

In a former paper, *Monthly Notices* liv. 7, p. 417, Mr. Dyson and myself called attention to the important effect that an error of the nature of Chandler's latitude variation would have on the values derived for the correction to the right ascensions of all stars, or to the value of the obliquity according as its maximum or minimum was near the equinoxes or solstices.

The latitude variation as determined by Dr. Chandler is the resultant of two periods, one annual, the other of about 427 days, varying slightly in period and amplitude, the value of which for Greenwich is determined from the expression (*Astronomical Journal*, xv. 360)—

$$\phi_1 - \phi_0 = r_1 \cos(t - \tau_1)\,\theta + 0''\cdot11 \cos \odot.$$

where ϕ_1 and ϕ_0 are respectively the values of the instantaneous and mean latitude.

$$\tau_1 = 2402327 + 428\cdot6\ \text{E.} + 55 \sin \psi.$$

$$r_1 = 0''\cdot135 + 0''\cdot050 \sin \psi.$$

$$\psi = (t - 2402327)\ 0^\circ\cdot015.$$

θ = the longitude of the fictitious or mean Sun.

Here also will be found a small table giving the values of τ_1, r_1, and θ for each value of E. for the years 1888–1900, and the latitude variation for every 20 days for the years 1893–1896 : in *Astronomical Journal*, xvii. 392, is a table of latitude variation for 1897, and in *Astronomical Journal*, xviii. 426, one for 1898.

In the following table are given latitude variations for the middle of each month as corrections to North Polar distances for the years 1890–1898, the factor for converting the corrections into ecliptic North Polar distance, and in the last column the correction for the annual term alone ($+0''.11 \cos \odot$).

Table of Corrections to N.P.D.'s for Latitude Variation for the Middle of each month for the years 1890–1898.

Day.	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.	1898.	Factor.	Annual Term.
	″	″	″	″	″	″	″	″	″		″
Jan. 15	+·21	+·18	+·06	−·06	−·10	−·04	+·08	+·17	+·18	·98	−·05
Feb. 14	+·22	+·25	+·17	+·04	−·05	−·04	+·06	+·16	+·22	·94	−·09
Mar. 15	+·18	+·27	+·24	+·13	·00	−·04	+·02	+·13	+·22	·92	−·11
April 15	+·09	+·21	+·25	+·19	+·05	−·03	−·03	−·06	+·17	·93	−·10
May 15	−·01	+·12	+·21	+·19	+·08	−·03	−·08	−·03	+·08	·97	−·06
June 15	−·12	−·01	+·13	+·16	+·09	−·02	−·12	−·12	−·03	1·00	−·01
July 15	−·21	−·14	+·02	+·10	+·09	−·02	−·13	−·18	−·14	·99	+·04
Aug. 15	−·22	−·20	−·07	+·02	+·06	·00	−·11	−·21	−·21	·94	+·06
Sept. 15	−·24	−·27	−·19	−·06	+·03	+·02	−·07	−·18	−·24	·92	+·11
Oct. 15	−·17	−·24	−·24	−·12	·00	+·04	−·01	−·10	−·21	·93	+·10
Nov. 15	−·07	−·17	−·23	−·15	−·02	+·07	+·07	−·02	−·13	·97	+·06
Dec. 15	+·07	−·04	−·17	−·14	−·04	+·08	+·13	+·10	−·02	1·00	+·01

Assuming that the error in ecliptic North Polar distance can be represented by the expression—

$$x \times \cos \text{Sun's longitude} + y \times \text{Sun's longitude} + z,$$

and that the Sun's observations for each month can be fairly represented by the middle date of each month, then giving equal weights to each month, it is easy to show that Dr. Chandler's annual term alone produces the following corrections :—

$$x = -0''.10$$
$$y = \quad ·00$$
$$z = \quad ·00$$

The right ascensions of all stars therefore require to be diminished by $\frac{0^s.10}{15 \sin \epsilon}$ or $0^s.016$. This quantity is a constant for all years.

In the same way it can be shown that the effect of the 427-day period for the years 1890–1898 is as follows :—

	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.	1898.
x =	−″·09	−″·15	−″·11	+″·01	+″·11	+″·14	+″·07	−″·04	−″·13
y =	+″·12	+″·03	−″·10	−″·13	−″·08	+″·03	+″·11	+″·12	+″·05
z =	+″·02	−″·01	−″·01	−″·02	−″·02	″·00	+″·02	+″·02	+″·01
x =	−ˢ·016	−ˢ·026	−ˢ·019	+ˢ·001	+ˢ·018	+ˢ·023	+ˢ·011	−ˢ·007	−ˢ·022

15 sin ϵ

The effect of the 427-day period is therefore to produce a seven-year period in the observed corrections to the right ascensions of the stars derived from the observations of the Sun, as also in the deduced value of the obliquity (y) similar in amplitude, but varying in epoch, whereas z or the observed distance from the pole to the ecliptic, is not sensibly disturbed.

The 427-day period, as determined by Dr. Chandler, is liable to small fluctuations in amplitude and period, and therefore the above corrections are liable to similar changes for other dates.

On the "Two-Method" Personal Equation.

By Walter W. Bryant.

For several years past it has been part of the routine at Greenwich for the transit observer for the night to observe occasional clock-stars by the eye-and-ear method, so that, in the event of a breakdown of the chronograph or of the observing circuit, all the observers might be in sufficient practice to be able to take good observations by the older method, and also that their personal equations, when using that method, might be approximately known.

Before 1892 it was the rule to deduce a separate clock-error from stars observed eye-and-ear (generally not more than two by one observer on the same night), and to compare this with the clock-error deduced from all the galvanic observations of clock-stars on the same night by the same observer. But as this involves an extra element of uncertainty in the comparison, it was decided in 1892 to compare only the observations made of the same star by the two methods on the same night, thus at once very much simplifying the comparison, and eliminating instrumental error.

Professor Safford has already made use of some of the results in papers presented to the Society, but, as my investigation starts from a different point, and as I am enabled, by the permission of the Astronomer Royal, to include some hitherto unpublished

results for more recent years, I hope to add some interesting matter on this subject.

I had intended to deal with the whole period of six years during which the same rule has been followed, but I find it better to omit 1892, as, of the three observers who have worked regularly since that time with the transit circle, H. was absent on longitude work for a great part of 1892, and A. C. and B. were both in that year too recent as observers to have formed a steady "habit."

First let me note briefly what appears to me to be the psychological meaning of the interval under discussion.

In galvanic observations there are two known methods, called by Professor Newcomb methods A and B, and referred to by psychologists as muscular and sensorial. In the first the observer's attention is concentrated on effecting a coincidence in time between his perception of the bisection of a star, and his making the galvanic contact. In the second the observer waits until the moment of estimated bisection before making contact. Hence, omitting accidental errors, depending partly upon the galvanic current itself and partly on the clock, I should expect the time of a transit to be $t+t_s+t_c$ by method B, and $t+t_s$ by method A, t being the true time, t_s time occupied in seeing or "reaction to light," t_c in making contact; which in method A is to be theoretically zero.

In general, then, I should expect observers by method A to register a little late, and those by method B later still, by the addition of t_c.

In eye-and-ear observations a new suffix is introduced, t_h instead of t_c, where t_h is what the psychologists call the "reaction to sound," hence in this method I should expect, omitting as before accidental errors,

$$\text{Time of Transit} = t+t_s-t_h.$$

There are two methods of eye-and-ear observing, but I see no reason to suspect any difference in the results comparable with the known difference between the two galvanic methods.

Thus the two-method personal equation should be fairly represented for method A by $+t_h$ and for method B by $+t_h+t_c$.

It is to be expected, therefore, that, except in very abnormal cases, observers will make their eye-and-ear transits earlier than galvanic ones, and that those who adopt method B (of whom H. is a well-known example at Greenwich) will have a much larger discordance in the same direction.

It must be borne in mind in comparing the tables which follow that in general the transit-clock is not the same as the sidereal standard used for galvanic observations. Clock Hardy in the transit-room is practically the only one in use for the purpose of eye-and-ear comparisons, and Clock Hardy is a long-suffering machine, exposed to many climatic changes, and with people

often passing to and fro, close to its face, so that it is not fair to expect a constant rate to be maintained. Hence it is vital that a comparison between the two clocks should be made as nearly as possible at the same time as the eye-and-ear observation, if the investigation is to deal with any confidence in very small intervals of time.

It is also important that such comparisons should be made always alike. Clock Hardy is provided with an automatic registration circuit for this purpose, but it sometimes fails owing to bad contacts, faulty insulation, or other causes. So that in many cases some hours elapse between the eye-and-ear observation and the Hardy signal, and in many other cases the signal is sent by hand, in which case the greater part of t_h is liable to disappear, thus reducing the apparent two-method personal equation.

This happened for a long period in 1893, owing to some defect in the wires, and will be referred to later.

It has also sometimes happened that the seconds of Clock Hardy have been alternately long and short, which would cause a systematic error to creep into observations of stars whose eye-and-ear interval is an even number of seconds or thereabout, while those whose eye-and-ear interval is an odd number of seconds would be practically unaffected.

Add to these many other probably small sources of uncertainty, and it will be seen that even if the number of comparisons is large enough to exclude unsystematic errors, it is quite possible that the results will be disappointing.

The following table gives the two-method equation for H., A. C., and B., in each of the first years under discussion, with the number of comparisons and the probable error of a single determination (which has been taken as the unit instead of a single night's determination) :—

	1893.	1894.	1895.	1896.	1897.
H....	$+^s\cdot441$	$+^s\cdot481$	$+^s\cdot438$	$+^s\cdot527$	$+^s\cdot508$
No. of Stars ...	78	40	80	39	45
Probable Error ...	$\pm^s\cdot062$	$\pm^s\cdot082$	$\pm^s\cdot086$	$\pm^s\cdot085$	$\pm^s\cdot053$
A. C.	$+^s\cdot107$	$+^s\cdot093$	$+^s\cdot062$	$+^s\cdot071$	$+^s\cdot082$
No. of Stars ...	69	24	22	17	25
Probable Error ...	$\pm^s\cdot065$	$\pm^s\cdot051$	$\pm^s\cdot059$	$\pm^s\cdot065$	$\pm^s\cdot072$
B.	$-^s\cdot004$	$+^s\cdot056$	$+^s\cdot040$	$+^s\cdot016$	$+^s\cdot025$
No. of Stars ...	196	109	124	141	85
Probable Error ...	$\pm^s\cdot064$	$\pm^s\cdot057$	$\pm^s\cdot051$	$\pm^s\cdot046$	$\pm^s\cdot041$

Mean values ... H. = $+^s\cdot479$ A.C. = $+^s\cdot083$ B. = $+^s\cdot027$.

The relative smallness of the values for H. and B. in 1893, and for H. and A. C. in 1895, I am inclined to attribute to the

failure of automatic registration mentioned above. With these exceptions there is no large annual variation, and, in fact, nothing that might not be accidental owing to some of the causes explained. I have, however, gone further into the subject from three points of view.

(1) Dividing each year into months.
(2) Dividing each day into 3-hour groups.
(3) Grouping the stars for every 10° of N.P.D.

The first of these is done for B. and H., the others for B. only, as there seemed to be insufficient data for a complete discussion of all three.

	H.			B.		
	Two-Method P.E.	No. of Stars.	Probable Error.	P.E.	No. of Stars.	Probable Error.
	s		s	s		s
January	+·443	12	±·099	+·034	53	±·055
February	+·425	24	±·090	+·004	44	±·066
March	+·454	30	±·063	+·015	68	±·060
April	+·451	31	±·075	−·021	53	±·053
May	+·456	39	±·081	+·022	69	±·055
June	+·471	26	±·071	+·031	42	±·058
July	+·520	19	±·113	+·040	31	±·051
August	+·455	22	±·062	+·022	66	±·050
September	+·498	17	±·055	+·032	57	±·046
October	+·489	19	±·056	+·021	65	±·055
November	+·500	21	±·076	+·043	77	±·056
December	+·502	22	±·089	+·023	38	±·042

G.M.T.	0^h	3^h	6^h	9^h	12^h	15^h	18^h	21^h
P.E. of B. ...	−s·003	+s·028	+s·006	+s·024	+s·030	+s·037	+s·034	−s·018
No.	3	45	160	166	125	89	48	21
Probable Error	±s·077	±s·062	±s·053	±s·055	±s·059	±s·040	±s·058	±s·055

N.P.D.	51°–61°	61°–71°	71°–81°	81°–91°	91°–101°	101°–111°	111°–121°
P.E. of B. ...	−s·002	+s·033	+s·020	+s·029	+s·019	+s·019	+s·006
No.	56	126	154	155	94	54	17
Probable Error	±s·064	±s·054	±s·050	±s·050	±s·051	±s·061	±s·070

As I anticipated in the early part of the paper the result should be persistently positive, it may be well to note the cases where it is negative, or so small as to be of doubtful sign. I find that a very large proportion of the observations included in B. February, B. April, and B. 6^h were taken in 1893. It has occurred to me to reject 1893, but the evidence is insufficient as to whether on any occasion the registration of signals was or was not automatic. So I have allowed it to stand.

The cases B. 0^h, B. 21^h, and B. 111°–121° rest upon very few observations, and probably do not give a real mean value at all.

There remains only B. 51°–61°, but I find that a very unfair proportion of these are of *α Lyræ*, which brings me to another line of investigation.

It is said that there is an almost invariable tendency to observe (galvanically) bright stars relatively earlier than faint ones, possibly owing to a larger bisection error which may be systematic. Into this question, however, I have no wish at present to intrude, as it does not come within the scope of the paper. I only allude to it because if true the anomalous nature of the result, B. 51°–61°, might be fairly ascribed to the supposition that for *α Lyræ* in particular the galvanic time was early and hence the relative P.E. too small.

I should have been glad to carry out the comparison between groups of stars of different magnitudes ; but there is practically no reliable evidence as to the observing conditions in each case, and as to the relative brightness of the same star under different conditions of daylight, twilight, darkness, and cloud.

One other field remains, not foreign to the subject. In observing slow-moving stars, I have practically no data to go upon, but, so far as my experience goes, stars right up to the pole should not give more widely different results between the two methods than the clock-stars for which the data are at hand.

For stars within a few degrees of the pole, I am inclined to think that the Greenwich method of galvanic observations with the very slight modification necessary to make it applicable to eye-and-ear observations, by substituting a known second from the clock for the galvanic contact simultaneously with which the bisection is made by means of the Right Ascension micrometer, would give practically the same result by either method.

A Note on the Result concerning Diffraction Phenomena recently criticised by Mr. Newall. By F. L. O. Wadsworth.

(*Communicated by the Secretaries.*)

In the November number of the *Monthly Notices* Mr. Newall has a note calling attention to an error in a result recently used by me in developing the theory of the "contrasting" or "delineating power" of telescopes. A criticism of this same result had already been published by Professor Schaeberle in the *Astronomical Journal* (No. 421). The criticisms of both Mr. Newall and Professor Schaeberle are just so far as the inaccuracy of the result alone is concerned ; but they are both at fault as to the real error that was committed in obtaining it, and as to its influence on the conclusions of my former papers.

This error was discovered by me several days before I had seen either Mr. Newall's or Professor Schaeberle's article.* A note has been published in the *Astronomical Journal* (No. 424) pointing out the errors in Professor Schaeberle's criticisms ; in the present communication I wish to point out other errors in certain statements by Mr. Newall.

A. At the beginning of his note Mr. Newall refers to certain papers of mine in a way that leads one to infer (1) that they were merely duplicate publications in different journals ; (2) that the conclusions of all these papers were affected by the error in the expression for I^2_{111}, the focal plane illumination due to the light from the sky. The first point is not perhaps an important one, but I should like to say a few words in regard to it. My preliminary note in the *Monthly Notices* (1897 June, pp. 586–9) simply pointed out two facts whose importance, in photographic work at least, had not, apparently, been fully recognised—*i.e.* that the general illumination of the sky, due to the scattering of light by small particles in our atmosphere, is (*a*) of much greater importance in photographic than in visual work, because the intensity of the scattered light varies inversely as the fourth power of the wave length ; (*b*) that this illumination can be considered as equivalent to an infinitely extended uniformly luminous area ; and that it is the *contrast* between the general illumination of the focal field due to such an area and the intensity of the image of any object, rather than the absolute value of the latter quantity, that determines the "visibility" and to a large extent the "definition" (both visual and photographic) of faint objects. In that note I also gave, without proof, some of the conclusions which I had then reached in the development of this general theory of "contrast" and "delineating power," based on these and other results. The next publication referred to was that in the *Astrophysical Journal* (1897 August, pp. 119–35), in which the general preliminary development of this theory was taken up from a purely analytical side. It was intended to make use of the results obtained, in conjunction with those derived from the well-known theory of "resolving power," in a discussion of the best practical instrumental conditions of working in the following special cases :—

(*a*) Nebular and stellar photography.
(*b*) Nebular and stellar spectrography.
(*c*) Coronal photography during and without an eclipse.
(*d*) Photography of solar, lunar, and planetary surfaces.
(*e*) Meteor photography and spectrography.
(*f*) Visual observations of fine markings on lunar and planetary surfaces.
(*g*) Visual observations of faint comets, nebulæ, the Gegenschein, zodiacal light, &c.

* *Astrophysical Journal*, 1897 December, p. 463 ; ibid. 1898 January, p. 77.

(*h*) Measurements of angular diameters and distances with the micrometer, heliometer and interferometer.

In each of these cases the conditions of working differ to a considerable degree ; and although much has been written on each, there still seemed to be some points that called for further discussion. (*a*) was discussed in the *Astronomische Nachrichten* (No. 3439) (this paper was afterwards published in *Knowledge*); (*d*) in the *Observatory* (1897 September, October, and November, pp. 303, 365, 404) ; and (*f*) in the *Astronomical Journal* (No. 414) ; the remaining cases have not yet been taken up in detail. These papers are not therefore duplicate publications, as intimated by Mr. Newall.*

B. With reference to point (2) Mr. Newall states that my conclusions "concerning the performance of various lenses" are based "on the misinterpretation of a result obtained by Stokes," and that consequently my "conclusions are incorrect," and my "criticism and attempted explanation . . . of practical results obtained with large and small reflectors and refractors falls to the ground." These statements again are incorrect. In the first place, the result to which Mr. Newall refers (the expression for I^2_{111}) *was not obtained by Stokes* (although he gave an analytical proof of the value of the integral involved), but by Lord Rayleigh, who announced it (for the first time, I believe), in his memoir on the Wave Theory in the ninth edition of the *Enc. Brit.*, as follows :—"If we integrate (30)—*i.e.*

$$d\xi \int_{-\infty}^{+\infty} I^2 d\eta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

with respect to ξ, between the limits $+\infty$ and $-\infty$, we obtain πR^2, as has already been remarked. This represents the whole illumination over the focal plane, due to a radiant point whose image is at O,† *or reciprocally the illumination at* O (*the same as*

* The practice of publishing a paper simultaneously in several journals is one that is quite common on this side of the water. Personally, I have followed it only a few times (three altogether, including the recent case in *Knowledge*[1]); but I do not consider it necessary or in good taste to either defend or condemn it. It should be said, however, that the reason for it, when it is done, is not that usually imputed to us by our transatlantic friends, but is simply the desire to reach our co-workers in the same field in this country. There are very few of our institutions which have even *one* complete file of the leading scientific publications in any subject; not a single one, as far as my experience goes, that has complete files of *all*. Many munificent gifts have been made for buildings and large instruments, but the fact has not yet impressed itself upon either the American public or upon our Boards of Trustees, that in order to carry on the work of an institution effectively, a good library and equipment of minor instruments is quite as important and essential, if not more so, than a fine building and (for example) a big telescope.

† O is the centre of the field.

[1] In a number of cases my papers have been *reprinted* in other journals. This is quite a different case, and one in which the editors of the journals are solely responsible and solely concerned.

at any other point), *due to an infinitely extended luminous area.*" * A corresponding result is reached earlier in the same memoir for the intensity of illumination in the image of a long line.*

In the second place, the conclusions in my different papers are based not upon this one result, but, as already pointed out, upon the indications of the general theory of "contrast" or "delineating" power. In two of the three cases, *i.e.* (*d*) and (*f*), that have so far been considered, the result does not enter *at all*, and Mr. Newall has not therefore any right whatever to refer to these cases as affected by the error in the expression for I^2_{111}. The only case (yet considered) in which it does enter is (*a*). The conclusions reached in this case would have been erroneous had it not been for the fact that in reaching them I omitted (fortunately or unfortunately) to consider the effect of atmospheric aberration on the *effective intensity of point and small surface areas during prolonged photographic exposures*. This effect had not been lost sight of, but its importance in (*a*) was at first under-estimated. It was intended to take up at once the general investigation of this effect as Part II. of the "General Theory of Telescopic Images" (of which Part I. was published in the *Astrophysical Journal* 1897 August), but the investigation had to be temporarily laid aside.† When I did recently take it up again, I at once discovered that the effect of this factor *in prolonged photographic exposures* would be in general to diminish the *effective photographic intensity* of *faint* point and small surface sources (stars, stellar nebulæ, &c.) in the ratio $\frac{1}{f^2}$, and of line sources in the ratio $\frac{1}{f}$.‡ If the result for I^2_{111} had been (as first assumed) independent of *f*, the contrast would therefore have varied as $\frac{1}{f^4}$ or $\frac{1}{f^3}$, instead of $\frac{1}{f^2}$, as found in practice. This discrepancy led me to reinvestigate Rayleigh's result and discover the error in the value for I^2_{111}. As it now turns out, the expressions for the *effective photographic contrast* between sky and faint stars, or faint diffused nebulæ, are practically correct as given in my preceding papers; the omission of the factor $\frac{1}{f^2}$ in the denominator (expressing the intensity of illumination due to the sky) just balancing the omission of the factor

* "Wave Theory," *Enc. Brit.* vol. xxiv. §§11 and 12. A reference to these paragraphs was given in both my preliminary note to the *R.A.S.* (*Monthly Notices*, footnote on p. 587) and in my subsequent paper in the *Astrophysical Journal* (footnotes on pp. 130, 133). I can hardly see how these could have so completely escaped Mr. Newall's attention. He has indeed completely overlooked the error in the expression for the intensity in the image of a long luminous *line*, although it is exactly of the same nature as that for an extended luminous *area*.

† See note in *Astrophysical Journal*, 1898 January, p. 78.

‡ For further discussion of this point see Part II. of "General Theory of Telescopic Images; Effect of Atmospheric Aberration," *Astrophysical Journal*, 1898 January, p. 70.

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$\frac{I}{f^2}$ in the numerator (expressing the effect due to atmospheric aberration). The main conclusions of the paper on stellar and nebular photography, (*a*), are therefore correct, as well as those on planetary observations, (*d*) and (*f*); *i.e.* at least so far as the theory upon which they are founded (and this has not as yet been questioned as far as I know) is correct. As regards this latter point it seems to me that the agreement between the actual times of exposure required to obtain a given photographic effect with different lenses (Barnard's and Pickering's results), and the computed times as determined by the theory of contrast, is too remarkable and striking to be considered as simply accidental and fortuitous.*

There is one minor conclusion of the paper in the *Astronomische Nachrichten* which is indeed based directly and solely upon the first (erroneous) expression for I^2_{111}, and which is consequently in error. This is the conclusion in regard to the "fogging" of the photographic plate by the sky illumination, and the consequent relation between the times of exposure possible with lenses of different apertures.†

Finally, just a word as to the actual error which was committed in obtaining the value of I^2_{111}, since this has not been pointed out by Mr. Newall. In the case of Lord Rayleigh the error was (apparently) due to the assumption (without proof) of the general principle of reciprocality or reversibility of images, *i.e.* that (as stated in my paper in the *Monthly Notices*)

$$\int_{-\infty}^{+\infty}\int_{-\infty}^{+\infty} I^2 d\xi d\eta \;.\; \equiv \;.\; \int_{-\infty}^{+\infty}\int_{-\infty}^{+\infty} I^2 dx dy \;.\;.\;.\;.\;.\;. \quad (1)$$

In my case (I approached the question from a somewhat different point of view) the error was a more purely analytical and hence, in many respects, a less excusable one. Mr. Newall is correct in saying "there is a discontinuity in my solutions." In the paper in the *Astrophysical Journal* I derived three expressions—(13), (16), (17)—for the most general case of an image of a source having any form, extent, or distribution in intensity. I applied the first of these—(13)—to the determination of the correct expressions for the intensity in the images of short lines and small uniformly luminous circular areas.‡ Further, when (13) was applied to the cases of a long line and an infinitely extended source, I also obtained correct expressions [(19) and (31)], but

* See *Ast. Nach.* No. 3439, § 100; *Knowledge*, 1897 August, p. 194.

† A note calling attention to this error has been sent to the *Astronomische Nachrichten*. I may perhaps point out in this connection that this result was not regarded as a particularly important one, and was not therefore included in the summary of conclusions at the end of the paper. I said in regard to it, "the mere ability to lengthen the time of exposure (at least beyond twenty-four hours) by decreasing the size of the objective would not in itself be of great importance."

‡ These results were those used in the discussion of cases (*d*) and (*f*).

I assumed that these expressions (in which the variables of integration were x and y, and the field of integration the radiating source) became identical [see (1) above] with the corresponding ones [(8) and (30)]* used by Rayleigh (in which the variables of integration are ξ, η, and the field of integration the focal plane). This would be the case when the limits of integration are infinite, if (as at first appears) the variables x, y were *symmetrically* involved with the variables ξ, η, in the expression for $I^2{}_1$. An inspection of (14) (of my paper), which expresses the value of r (the variable in I^2) in terms of x, y, ξ, η, shows that this is not the case. The first two variables are each multiplied by a factor of dissymmetry, $\frac{f}{D}$, and in order to obtain two new variables symmetrically involved with ξ, η, we must substitute

$$x = \frac{D}{f} d\xi_1, \qquad y = \frac{D}{f} d\eta_1,$$

which gives us in (19) and (31) respectively,

$$\int_{-\infty}^{+\infty} I^2 dy = \frac{D}{f} \int_{-\infty}^{+\infty} I^2 d\eta_1 = \frac{D}{f}(z_{11}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (19a)$$

and

$$\int_{-\infty}^{+\infty}\int_{-\infty}^{+\infty} I^2 dx dy = \frac{D^2}{f^2}\int_{-\infty}^{+\infty}\int_{-\infty}^{+\infty} I^2 d\xi_1 d\eta_1 = \frac{D^2}{f^2}(z_{111}) \quad . \quad . \quad . \quad (31a)$$

and the integrals [(z_{11}) and (z_{111})] of *these last expressions* become respectively identical with the integrals (8) and (30) of Lord Rayleigh.

Yerkes Observatory: 1897 *December* 24.

The Concave Grating for Stellar Photography. By Charles Lane Poor and S. Alfred Mitchell.

(*Communicated by Dr. C. L. Poor.*)

The concave grating has proved a most powerful instrument for spectroscopic research, but heretofore it has not been successfully applied to stellar spectroscopy. Experiments are now being carried out at the Johns Hopkins University, under the direction of Dr. Charles Lane Poor, with the view of thoroughly testing the various methods of using the concave grating for astronomical purposes. The methods, originally suggested by Professor Rowland, were developed and the formulas derived by Dr. Poor, and the apparatus constructed under his direction; the photographs were made by Mr. S. Alfred Mitchell.

* "Wave Theory," *Enc. Brit.* §§ 11 and 12.

There are two radically different methods of using the concave grating for stellar work :

First, in connection with an objective ; the grating merely replacing the ordinary stellar spectroscope. This was tried by Professor Crew at the Lick Observatory in 1892 and a few results obtained.

Second, direct ; the grating is the objective and the spectroscope combined : the light from the star being reflected directly from the grating to the photographic plate. This method was first developed by Dr. Poor in 1892, when a few experiments were made. No definite results were then obtained, however. Experiments were resumed in October last, and some promising photographs have since been obtained.

This paper is confined to an explanation of the second method, to the derivation of the necessary formulas, and to a few notes in regard to the photographs already taken.

From the theory of the concave grating we have the general equation,

$$r = \frac{\mathrm{R}\,\rho\cos^2\mu}{\mathrm{R}\,(\cos\mu+\cos\nu)-\rho\cos^2\nu} \qquad (1)$$

(see Rowland, *American Journal of Science*, vol. xxvi. 1883 Aug.). In this equation ρ is the radius of curvature of the grating, and the axis of the grating is the line of reference for angular measurements. R and ν are the spherical coordinates of the source of light ; r and μ those of the curve on which the spectra are brought to a focus.

This equation may be put into the following form :

$$r = \frac{\rho\cos^2\mu}{\cos\mu+\cos\nu-\frac{\rho}{\mathrm{R}}\cos^2\nu} \qquad (2)$$

If now the source of light is at an infinite distance, R is infinite, and the equation reduces to

$$r = \frac{\rho\cos^2\mu}{\cos\mu+\cos\nu} \qquad (3)$$

This is the general equation for the case under consideration.

We may use the grating in a number of different ways, depending upon the position of the photographic plate, and of the source of light in reference to the axis of the grating. One position is by far the best for photographic work, and in this note the formulas for that case alone are given. This position is that in which the centre of the photographic plate is on the axis of the grating.

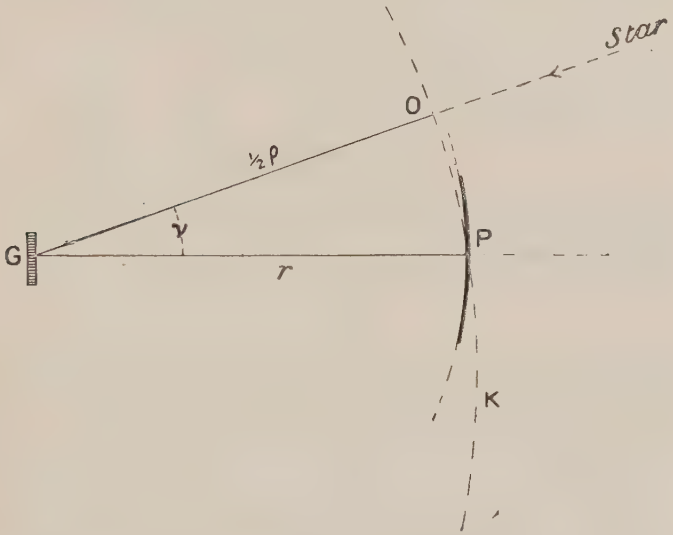
To investigate this case fully, return to the general equation, (3). For a fixed value of ν all the spectra are brought to a focus on the curve,

$$r = \frac{\rho\cos^2\mu}{\cos\mu+\cos\nu}$$

in which cos ν is constant. When μ is so small that its cosine may be taken as unity, this equation reduces to

$$r_0 = \frac{\rho}{1 + \cos \nu} = \text{a constant},$$

and the focal curve becomes a circle. This case is shown in the following figure, in which G is the grating and P the photographic plate.



The light from the star comes in in the direction of OG. The curve, OPK, is a parabola. OG being the half-parameter and equal to $\frac{1}{2}\rho$. For a constant value of ν, those spectral lines on the photographic plate, for which cos μ can be assumed equal to unity, are brought to a focus on a circle whose radius is r. When the dispersion is large, so that cos μ must be taken into account, the focal curve is as defined by equation (3), and the photographic plate must be bent along this curve.

By the theory of diffraction (Rayleigh, *Encyc. Brit.*, "Wave Theory of Light")

$$\lambda = \frac{\omega}{N} (\sin \nu + \sin \mu) \qquad (4)$$

where ω is the grating space and N is the order of the spectrum. From this we have at once

$$\frac{d\lambda}{d\mu} = \frac{\omega}{N} \cos \mu$$

To find the change in wave length as we pass along the focal curve, we have

$$\frac{d\lambda}{ds} = \frac{d\lambda}{d\mu} \cdot \frac{d\mu}{ds}$$

and

$$\frac{d\mu}{ds} = \frac{1}{\sqrt{r^2 + \left(\frac{dr}{d\mu}\right)^2}}.$$

Differentiating the equation of the focal curve, we find

$$\frac{dr}{d\mu} = \frac{r \sin \mu - \rho \sin 2\mu}{\cos \mu + \cos \nu} \equiv \phi(\mu) \tag{6}$$

Whence substituting, we finally find

$$\frac{d\lambda}{ds} = \frac{\omega}{N} \cdot \frac{\cos \mu}{\sqrt{r^2 + [\phi(\mu)]^2}} \tag{7}$$

and this is the general formula for the change in wave length along the focal curve.

If, in this equation, we put $\mu = 0$, it reduces to—

$$\frac{d\lambda}{ds} = \frac{\omega}{N} \cdot \frac{1}{r_0} \tag{8}$$

a constant. Hence at this point the spectrum is "normal." Within the limits, therefore, in which we can take $\cos \mu$ as equal to unity, the spectrum may be considered as normal.

For a grating of medium dispersion an entire spectrum will be practically normal, provided that the centre of the plate is on the axis of the grating—*i.e.* μ equal to zero for the middle of the spectrum. In the grating used in our experiments the entire first order spectrum subtends an angle of less than 6°. By computation we find from the above formulas that the scales of different portions of the spectrum differ by less than three parts in a thousand. The advantages of this method of working are thus apparent.

In order to test the above method, a small Rowland concave grating with a ruled surface of 1×2 inches was used. The grating has a radius of curvature of one metre, and is ruled with 15,000 lines to the inch. The apparatus for mounting the grating is extremely simple, consisting of a light box clamped to the tube of the equatorial—the telescope being used merely as a finder. The light from the star falls directly on the grating, is diffracted and brought to a focus on the photographic plate. The grating is mounted in an ordinary holder, which can be adjusted by side and back screws. The plate-holder holds a plate 1×5 inches, bent as closely as possible to the proper curve. The holder is capable of adjustments, so that the plate and the grating can be made parallel. The box is clamped to the telescope in such a way that the lines of the grating are parallel to

the equator; and accordingly, by regulating the driving clock of the telescope to run a little too slow or too fast, the spectrum can be made of any convenient width.

For our trials *Sirius* was the star principally used, and the exposures ranged from ten minutes to one hour, according to the width of the spectrum. All the photographs were made with the first order spectrum, and Seed's gilt-edge plates were used. The spectra are about 5 cm. long, and vary in width from 0·1 mm. to 1·5 mm., depending upon the exposure and the rate of the clock. Details of a few specimen plates follow:

Sirius.—November 27. Exposure 40 minutes; width of spectrum, 1·5 mm. Plate shows eight hydrogen and H and K lines.

Capella.—December 9. Exposure 40 minutes; width of spectrum, 0·2 mm. Plate shows F, G, *h*, H, K, and about fifty fine lines.

Procyon.—December 15. Exposure 40 minutes; width of spectrum, 0·15 mm. Plate shows six hydrogen, H, K, and about twenty fine lines.

Rigel.—December 28. Exposure 85 minutes; width of spectrum, 0·1 mm. Plate shows fourteen hydrogen, H, K, and six other lines.

Sirius.—January 3. Exposure 40 minutes; width of spectrum, 0·1 mm. Plate shows sixteen hydrogen, H, K, and fifteen other lines.

The glass positive which accompanies this paper is the enlargement of two of our negatives of the spectrum of *Sirius*. The lower one is the enlargement of a plate taken 1897 December 15; the upper spectrum is the enlargement of the plate taken 1898 January 3 (see above). Since our original negatives are extremely narrow, considerable trouble was experienced in widening out the spectra without introducing spurious lines. Although some of the finer lines in the enlargement are undoubtedly spurious, yet the plate shows the general character of our photographs, in that it shows clearly the hydrogen lines, K, and many other lines which can be easily identified.

All these experiments were carried out in the Observatory, which is most unfavourably situated for such work. We are confident that much better results will be obtained under better conditions, and think that this method promises to become of great value to stellar spectroscopy.

Johns Hopkins University:
1898 *February* 5.

On a Convenient Method of adjusting a Polar Axis to the Diurnal Motion. By David P. Todd.

[*Communicated by the Secretaries.*]

To eclipse observers often occurs the practical problem of adjusting a polar axis to diurnal motion in the briefest possible time. As a rule their mountings will have no circles, or very small ones ; so that the usual method of adjustment either is inapplicable or falls short of the precise alignment desired. Besides this, the finally adjusted axis points in the direction of the true pole, whereas, for best following by clock-motion, it should point toward the apparent or refracted pole, as indicated by Mr. Davidson in his recent paper.

Some years ago, when equipping the U.S. expedition to West Africa for the eclipse of 1889 December 22, I hit upon a method of speedy adjustment which has not, I think, been described heretofore, and which others may find as convenient and expeditious as I have repeatedly since that time. The mechanical requisite is a simple one ; the astronomical requisite is that the poleward horizon must be unobstructed—close circumpolars at least must be visible.

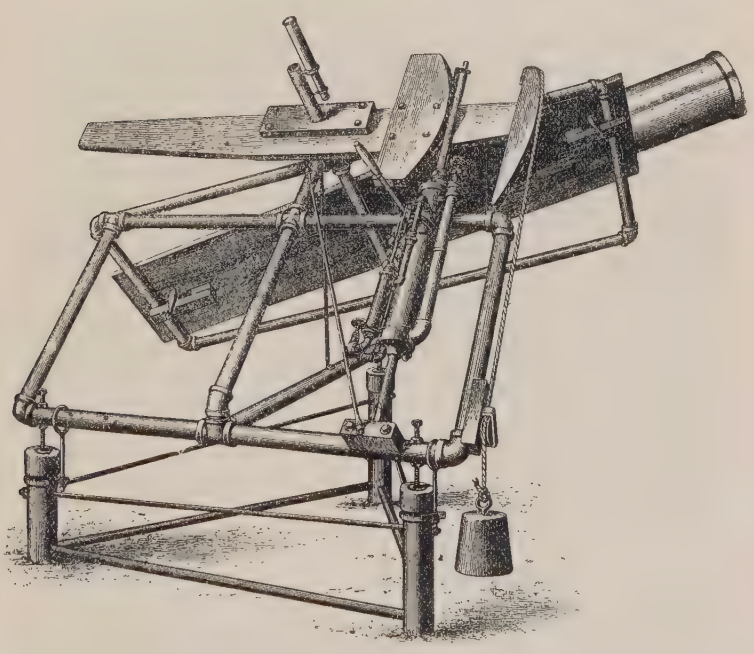
It so happened that this latter precluded actual use of this method on the expedition in question ; for, obliged as we were to locate in low south latitudes, proximity of a slight southerly eminence cut off our poleward view. But in the expedition to Japan in 1896—provided by the liberality of Mr. D. Willis James, a trustee, and his son Arthur Curtiss James, a graduate of Amherst College—the method was adapted to three equatorial mountings with perfect success. A lesser one is shown in the accompanying figure.

At the upper or left-hand end of the polar axis is shown a small finder, rigidly attached. The axis of it is adjusted parallel to the mechanical axis of rotation of the polar axis in its bearings. This is easily done by collimating on a well-defined object so distant that twice the space between telescope and polar axis is not a considerable part of distance of object itself.

The finder's large field shows an annular reticle, the ring's angular radius being equal to the polar distance of *Polaris*, if the mounting is to be adjusted in the northern hemisphere. Periphery of the ring is graduated counter-clockwise to twenty-four hours. I found by several trials that graduation to subdivisions of five minutes was fine enough. Preparation of those reticles was troublesome and expensive at first, because done with a dividing engine on glass. At last I found they could be made with high accuracy by photography for a few pence each : carefully draft the circle, graduation, and numerals on Bristol board in very fine lines, and reduce photographically, as if a lantern

transparency. The thinnest of cover glass was used; also the wet collodion process, because it yields a plate almost absolutely transparent.

The finder's focal length must be measured accurately, and the photographic reduction suited thereto. Then adjust the horizontal line of the reticle (18^h–6^h) parallel to the horizontal arm attached to the polar axis. See that this remains level by any convenient means, while the three adjusting screws under the mounting quickly bring *Polaris* into that part of the field



marked by its hour-angle on the graduated circle. A few moments' trial suffices for this adjustment; and verification at sight is always possible whenever the pole star is visible, the only requisite being a knowledge of sidereal time. In some cases a diagonal eyepiece will be an additional convenience. For the southern hemisphere a larger telescope is necessary, but a smaller field will suffice.

After practical use I find this method of adjusting a polar axis most convenient and desirable for expedition work, where disturbance after first adjustment is likely to occur. An accidental thrust in putting an instrument upon the mounting, or some unintentional blow from a rough labourer, may disturb a mounting so that readjustment is necessary. And the means of

constantly testing and quickly remedying this cardinal element of polar alignment is a great comfort as the hour of totality approaches, when a hundred other details crowd for instant attention.

Note on some Results obtained with a Small Prismatic Camera at the Eclipse Camp at Talni. By J. Evershed.

The eclipse work which I undertook in India this year was entirely spectroscopic, and covered practically the same ground as that which I attempted in Norway in 1896. It consisted in obtaining photographs of the spectra of the chromosphere, prominences, and corona.

For this purpose I constructed three photographic instruments : a prismatic camera of $2\frac{1}{4}$ inches aperture and 36 inches focal length, fitted with two 60° prisms ; a slit spectrograph, containing two quartz prisms ; and a large slitless spectrograph attached to a 6-inch telescope, the latter being mounted on a roughly made equatorial stand.

In addition to these I had available a 4-inch polar heliostat, kindly lent by Mr. Maw, and a $3\frac{1}{4}$-inch equatorial telescope with solar spectroscope attached. The heliostat was used to supply light to the prismatic camera and the slit spectrograph. It was of the ordinary form with two mirrors, but was modified to suit the special work by removing the second mirror and mounting it in the same plane as the first, so that two beams of light were available instead of one beam twice reflected. The tele-spectroscope was used to observe the reversing layer visually, and to determine the right moment to expose the prismatic camera and large slitless spectrograph in order to photograph the "flash" spectrum.

I obtained thirteen photographs in all : one only with the slit spectrograph, which was exposed during the whole time of totality ; two with the slitless spectrograph—one at the beginning and one at the end of totality ; and a series of ten with the prismatic camera, beginning about 20 seconds before second contact and ending about the same interval after third contact.

The single photograph obtained with the slit spectrograph shows no details of any kind on the very faint continuous spectrum of the corona. The large slitless spectrograph yielded two fairly good negatives, which show a considerable number of bright lines in the region of the spectrum between F and H. The best results were, however, obtained with the prismatic camera. This instrument gave images of the spectrum extending from λ 6000 in the orange to λ 3380 in the ultra-violet. Unfortunately the scale is very small ; it is only ·33 inch to the Moon's diameter, and in length 1·52 inches from D to H, the total length of the spectrum photo-

graphed being 2·9 inches. Notwithstanding this, however, a large amount of detail is visible on some of the plates, and the definition is excellent in the ultra-violet region.

The first two photographs of the prismatic camera series were exposed at about 20 seconds and 12 seconds before totality respectively. At these times the last remaining thread of sunlight had narrowed down to a width of 8″ for the first and about 4″ for the second exposure, at the widest part of the cusp, and, acting as a fine slit, gave excellent spectral images. Accordingly, the photographs show the ordinary Fraunhofer spectrum very finely, but the lines are much less dark than in the ordinary solar spectrum ; the F line in particular seems to be almost obliterated by the bright chromosphere radiation. H and K, and all the hydrogen lines except C, are represented in the photographs by bright arcs extending on each side of the continuous spectrum, and numerous other short bright arcs can be made out in the ultra-violet.

The next plate (No. 3) was exposed about 2 seconds before the last remnant of photosphere vanished, and was allowed to remain exposed for about 4 seconds. Just before making this exposure I removed the slit of the visual spectroscope and watched the narrowing cusp-spectrum in the region near *b* without any slit, beginning to observe not more than 10 seconds before totality.

At first I could see the Fraunhofer dark line spectrum very clearly, just as though the circular slit ordinarily used in observing the prominences was still in place. The continuous spectrum rapidly decreased in width, and then, instead of narrowing down to a single thread of light, as I had expected, it suddenly began to break up into a number of narrow bands, between which the bright lines flashed out in the most astonishing way, extending in long arcs over thirty degrees or more of the limb. I then made the exposure.

In the photograph only the stronger lines in the part of the spectrum observed have been impressed. In the ultra-violet, however, the spectrum is thickly crowded with bright lines. In this region the lines seem to be intrinsically more intense than in the lower spectrum, whilst the continuous spectrum of the photosphere is relatively feeble. All the bright lines throughout the spectrum, excepting those due to the chromosphere proper, form a well-defined band of uniform width running the whole length of the spectrum. The length of the arcs, or width of the band of bright lines, corresponds to an arc of over 40 degrees of the Sun's limb, and this implies a depth not exceeding 1″·5 for the gases composing the reversing layer.* The extreme thinness of the layer is evidenced also by the well-defined dark bands running lengthwise from end to end of the spectrum, and interrupting all

* Assuming the Moon's apparent semi-diameter = 997″·0, and the Sun's = 974″·9.

the bright lines of the flash spectrum. These are due to the projecting lunar mountains, which had already in places occulted the reversing layer, even before the eclipse was quite total.

Plate No. 4 was given an instantaneous exposure a few seconds after totality had commenced. It shows simply the ordinary chromosphere spectrum ; but there is a curious feature in the prominence spectrum which is well brought out in this photograph. In the extreme ultra-violet most of the prominences give an apparently continuous spectrum. This appears to commence abruptly at about λ 3660, at the point where the hydrogen series ends, and it extends in an unbroken line to the end of the plate, at about λ 3390.

Of the remaining photographs of the series, Nos. 7, 8, and 10 are the most interesting. No. 7 was given an exposure of about 30 seconds, ending just before third contact ; the corona line and many of the flash spectrum lines are clearly shown ; they are crossed by narrow streaks of faint continuous spectrum. The distribution of the " 1474 " light seems to conform in a general way to the structure of the corona ; but it is much stronger on the east limb than on the west, where it is scarcely traceable. In this photograph the entire series of hydrogen lines are shown, including α.

No. 8 was exposed at the moment when the photosphere broke out, and a large number of the flash spectrum lines are shown between the streaks of continuous spectrum. The focus is, however, not so good as in No. 3.

No. 10, which was exposed about 18 seconds after totality was over, is remarkable for the fringe of bright lines bordering the continuous spectrum in the ultra-violet, where, apparently, *every* dark line of the Fraunhofer spectrum ends in a short bright line.

With regard to the question of the relation between the bright lines of the flash spectrum and the dark lines of the Fraunhofer spectrum, it would be exceedingly rash to draw any definite conclusions from these negatives, pending the discussion of the magnificent results obtained by Sir Norman Lockyer and others. It may be stated, however, as the result of a preliminary examination of the plates, that while the flash spectrum cannot be regarded as the exact converse of the Fraunhofer spectrum, yet the correspondence between the two is a sufficiently striking one, and from it one might infer that the majority of the dark lines in the solar spectrum are really due to absorption in the reversing stratum.

But in studying the flash spectrum it is very necessary to bear in mind that, although apparently very shallow, the reversing layer is really something like 800 miles in depth, with probably an enormous increase of density near the base, or, rather, in the region where the photospheric clouds are precipitating.

Under such conditions the main part of the absorption producing the Fraunhofer spectrum probably takes place within a

few miles only of the photospheric clouds. (It does not seem necessary to suppose it to take place within or between the clouds, although this may, of course, be the case to some extent.)

The flash spectrum, however, as we see it, represents the higher part of the stratum, which is not nearly so effective in producing absorption, on account of the much lower density; and if the various gases concerned are not uniformly distributed throughout the layer, the relatively shallow but dense stratum at the base may contain gases which are not present in the region above, and *vice versâ*.

On this view, helium and other gases having prominent lines in the flash spectrum, which are only feebly represented, or are not present at all, in the Fraunhofer spectrum, are gases which exist only in the upper regions of low density. It is not necessary to suppose that the temperature of these gases is as high as that of the photospheric background, but simply that a ray of light, emitted by the photosphere in its passage outwards, encounters relatively few molecules, notwithstanding that the gases may extend to great elevations in the chromosphere.

It is evident that the strength of a bright line in the chromosphere or flash spectrum depends to a large extent on the height to which the gas producing it extends, and is independent of the density or total quantity of gas overlying the photosphere by which the intensities of the dark lines are mostly determined.

Thus, by assuming a somewhat irregular distribution of the gases in the layer itself, the differences between the Fraunhofer and flash spectra may be accounted for without abandoning the view that the greater part, if not the whole, of the gaseous absorption takes place between the photosphere and the upper limits of the reversing layer.

Note on the Zodiacal Light. By E. Walter Maunder.

The recent eclipse expedition to India gave me the first favourable opportunity for observing the Zodiacal Light. I had seen it on a few occasions in England, but the contrast between the faint glimmer, doubtfully seen on a few rare occasions here, and the broad bright glow, which it was impossible to mistake or overlook, seen on every moonless evening during both the outward and homeward voyage, and during our camp life in India, was so great as to seem to make it a duty to utilise such an opportunity as fully as possible. The observations cover a period of two calendar months—1897 December 22 to 1898 February 22—they are, of course, therefore of but little value as compared with a series of observations carried on continuously throughout the entire course of the year. Nevertheless I thought it worth while to submit them to the Society in the hope that they might call the attention

of observers in tropical stations to an object which has been as yet but little studied, and which demands no instrumental equipment.

The first point which struck me with regard to the Light was its brilliance, so far exceeding anything that I had anticipated. I estimated that the brightest regions of the Light, as seen in the evening, were fully six times as bright as the brightest regions of the Milky Way visible at the same time. The morning branch of the Light seemed to me hardly equal to the evening branch, and it was brought into rivalry with brighter regions of the Milky Way—namely, those in *Sagittarius*, *Crux* and *Argo*. I should incline to place it as about three times as bright as the brightest of these. Again, in the evening the Light made itself evident in the twilight considerably before the Milky Way was able to do so, and as seen at sea its reflexion on the water was much more marked.

The question at once occurred to me, is the Light always as bright as this in these latitudes, or are we being favoured with a specially brilliant display? If the former were the case, it seemed impossible to understand why so few people seem to have remarked it, and why the observations of it by astronomers are so rare and incomplete. I made what inquiry I could on this point and found a conflict of testimony; some observers assuring me that in India, at all events under latitude 20°, the Light was habitually as bright as I had seen it; others were just as positive that it had been unusually distinct at this particular season. My own experience, such as it is, would seem rather to support the latter view. Most assuredly the Light was in no such evidence in 1886 during our eclipse voyage to the West Indies, or I could not have failed to have noticed it. I may note that Father Marc Dechevrens of the Zi-ka-Wai Observatory, failed to observe the evening branch during the months of July to September, in the years 1875 to 1879.

It will, of course, be understood that the entire Light was not of this intensity. The brightest portion of the Light was spindle-shaped—the axis of the spindle being the brightest part, and the light shading off in all directions. But whilst the sides of the spindle were not so diffused as to render it impossible to trace their outlines with a fair amount of exactness, the Light at the point faded away so gradually that it was impossible to set a definite boundary to it there. Roughly speaking, the Light was distinctly brighter than the Milky Way for fully 30° of its length. At perhaps 45° it might be of about the same intensity. After this it degraded very rapidly, and when it crossed the Milky Way between *Taurus* and *Gemini* it was quite overpowered by the latter.

A more precise idea of the brilliance of the Light may be deduced from the fact that the young Moon was seen upon it on January 24 and February 22 without the Light suffering extinction. On both occasions it could be clearly made out. On the following

nights, January 25 and February 23, the increased moonlight was sufficient to overcome it.

Beyond the brighter spindle-shaped light a very faint band of light could be traced in a prolongation of the spindle nearly, if not quite, round the entire sky ;—at least it could be so traced at the end of December, for I had no opportunity of observing the morning branch of the Light after January 2. During the three weeks in camp the morning Moon interfered almost continuously with the observations of the eastern Zodiacal Light, and on the homeward voyage, though I rose nearly every morning between four and five o'clock, on no single occasion was the sky clear.

The failure to see the morning branch was the more disappointing as the evening branch appeared to shorten as time went on. On the very first occasion, *i.e.* on the evenings of December 22, 23, and 24, the evening branch was traced as far as ι *Tauri*. On February 21 it was scarcely seen so far—certainly no further, the elongation from the Sun being 164° in the first case, and only 102° in the second. It is possible that the brightness of the Milky Way, which crosses the ecliptic near here, was sufficient to drown the feeble glow of the Zodiacal Light at so great an elongation, and that but for its interruption it might have been traced as far from the Sun in February as in December. It was both broader and brighter in the neighbourhood of the Pleiades on the later occasion than on the earlier. As the observations given below indicate, the "spindle" was shorter and broader, was less oval, when last seen than when seen first.

The morning Light could not be traced quite so close to the Milky Way as the evening branch. *Præsepe* marked the furthest point to which it was followed to any degree of certainty. This on January 2 would correspond to an elongation of 156.°

The brightest portion of the Light showed a delicate but unmistakable colour—a faint pale yellow, with perhaps the slightest inclination to the green rather than to the orange. The contrast between this tint and the steely blue of the Milky Way was only less pronounced than the contrast between the broad areas of diffused luminosity of the Zodiacal Light and granulations of the Galaxy. Yet they had one feature in common. I felt convinced that in the Light as in the Galaxy there were dark lanes and rifts, though in the former these lanes are far more difficult objects, and I was not able to lay down the course of any of them with sufficient accuracy. Two lines of somewhat rapid fading, if not positive dark lanes, were suspected at right angles to the ecliptic near longitudes 16° and 25° respectively.

A feature of the Light which especially caught my attention was that it very nearly swamped the stars of the two streams in *Pisces* which radiate from α *Piscium*. The one stream running in a straight line from α to ρ *Piscium* is very nearly normal to the ecliptic ; the other, forming a gentle curve and running from α to γ *Piscium*, crosses the Light at a small angle.

The individual stars in these two streams, though many of them are fully of the fourth magnitude, were yet made out with considerable difficulty, especially those near the axis of the Light. I have had no opportunity of examining this part of the sky again since my return home, but so far as my recollection serves me these and other ecliptic stars in *Aquarius* and *Capricornus* were seen with more difficulty in the clear skies of the Red Sea and India than here at home, owing entirely to the great brilliance of the Light.

Beside the morning and evening branches of the Light, a very faint, vague luminosity was observed night after night from January 12 to January 18, lying between *Pollux*, *Procyon*, and *Præsepe*. It had at first seemed to be like a faint extension of the Milky Way, but on January 18 for the first time a clear interval was made out between the following side of the Milky Way and the preceding edge of this faint light. The Light itself was traced about 2° farther east than *Præsepe*. The centre, therefore, of the light was about longitude 112°. It was, therefore, as nearly as the observations allowed, in opposition to the Sun, and was no doubt what is now known as the "Gegenschein."

Below I give a series of outline points as determined on different occasions. It will be noticed that, so far as they go, they point to the Light being very nearly in the plane of the ecliptic; rather in that plane than in the plane of the Sun's equator. How far a more expanded series of observations would have confirmed this view, of course, I cannot say. The fact that the Light can be traced more than 90° from the Sun (indeed, if the Gegenschein be part of it, to actual opposition with the Sun), clearly proves that some portion of the Light is derived from matter lying outside the Earth's orbit. But by far the greater amount of the Light is concentrated in the spindle-shaped beam, and this although, as already stated, the place of its apex cannot be satisfactorily fixed, does not attain anything like so great an elongation, and it seems to me clear, if the Light were due to a disc of matter extending uninterruptedly outwards from the Sun to a considerable distance beyond the Earth's orbit, that the opposition portion would tend to appear much broader than is actually the case. The evening branch of the Light tapers from a breadth of 15° or 16° near the Sun, to one of 4° or 5° near opposition; and the morning branch is about 20° broad at its nearest observable approach to the Sun, and 4° at its greatest distance from it. But the particles between the Earth and the Sun must be "new," that is, invisible, whilst those either in opposition to the Sun, or on the further side of it, are "full." The fully illuminated particles are therefore much nearer to us in opposition, and consequently spread over a greater area, than those nearer superior conjunction, and if the Light were due simply to a continuous disc of particles extending outward from the Sun beyond the Earth's orbit, the portion of it opposite the Sun should be the broadest, not the narrowest.

On the other hand, if we suppose that such a disc of particles lies wholly within the Earth's orbit, its boundary extending say to 0·94 (corresponding to an elongation of 70°) of the Earth's mean distance from the Sun, we should have a glow of much the appearance and character of the brightest part of the Light, the "spindle-shaped" beam, the "Stronger Light" of Jones. The faint, narrow prolongation of the Light seen beyond an elongation of 70°, and traced even to opposition, showing as it does little variation in breadth, would seem to point to a flat ring of matter lying a good way outside the Earth's orbit. A satellite of *Saturn* revolving round its primary in the middle of the Cassinian division of the rings, would offer some analogy to the position of the Earth with regard to the two divisions of the Zodiacal Light.

When the region of the *Pleiades* is approached it seems very difficult to differentiate between the actual Light and faint, indistinct patches and arms of luminosity, which seem to be connected with and to radiate from the *Galaxy*.

But by far the brightest portion of the Light is satisfactorily accounted for by the usual hypothesis of a great disc of finely divided cosmical particles radiating from the Sun.

The plane of the disc seems to me very nearly to coincide with the plane of the ecliptic. The annexed observations made in February show that the Light then lay almost exactly along the ecliptic. The heliographic latitude of the Earth was then 7°·2 S. If, therefore, the disc extends to within a few million miles of our orbit, and if it lie in the plane of the Sun's equator, it should have shown an enormous displacement to the north of the ecliptic, which was most certainly not the case. In fact, the Light appeared if anything to lie further south in February, when the Earth was at its maximum south heliographic latitude, than in December when its latitude was only 3°.

I should like to add that I had read very little on the subject before leaving England, and had therefore not seen Professor Arthur Searle's suggestion for observing the Zodiacal Light by the method of contour lines. I greatly regret this, as the method, though requiring much practice, was obviously the proper one to have adopted.

Evening Zodiacal Light.

(1) *Observations made* 1897 *December* 22, 23, *and* 24.

Northern Border.	Southern Border.
2° N. α Capricorni	ψ Capricorni
ϵ Aquarii	ζ ,,
μ ,,	35 Aquarii
β ,,	3° N. δ ,,
1° S. γ Aquarii	1° S. ψ^3 ,,
1° S. η ,,	1° N. 30 Piscium

Northern Border.	Southern Border.
1° S. γ Piscium	1° S. μ Piscium
ι ,,	Midway between π and α Piscium
ω ,,	
η ,,	ξ Arietis
2° S. γ Arietis	Between 31 and 38 Arietis
Between ζ and δ Arietis	1° N. ϵ Tauri
1° S. Pleiades	ι ,,

(2) *Observations made* 1898 *January* 15.

β Aquarii	Just N. δ Aquarii
1° S. γ ,,	Just S. ψ^3 ,,
1° S. η ,,	30 Piscium
1° S. γ Piscium	1° N. 20 Ceti
ω ,,	ν Piscium
η ,,	Just N. ϵ Tauri
ζ Arietis	
Just S. Pleiades.	

(3) *Observations made* 1898 *February* 16, 19, 20 *and* 21.

2° N. γ Piscium	ι Ceti
1° N. ι ,,	6° N. η ,,
3° N. ω ,,	6° N. θ ,,
4° S. γ Pegasi	1° N. α Piscium
1° N. η Piscium	3° N. ξ Tauri
1° S. γ Arietis	Just N. ϵ ,,
Just N. Pleiades.	

Morning Zodiacal Light.

Observations made 1897 *December* 27 *and* 1898 *January* 2.

	ρ Scorpii
1° N. ξ Scorpii	39 Libræ
3° N. β Libræ	1° N 20 Libræ
3° N. δ ,,	4° S. λ Virginis
1° N. μ Virginis	$\frac{1}{2}$° S. α ,,
1° N. ζ ,,	3° S. γ ,,
δ ,,	2° S. η ,,
1° S. o ,,	1° S. β ,,
5° S. β Leonis	Between σ and τ Leonis
$1\frac{1}{2}$° S. θ ,,	ρ Leonis
1° N. η ,,	α ,,
γ Cancri	δ Cancri.

The approximate latitudes at the times of observation were :—

1897 Dec. 22	31° 0′ N. Lat.	1898 Jan. 15	20° 45′ N. Lat.
23	31 0	Feb. 16	13 30
24	26 0	19	21 30
28	13 30	20	27 0
1898 Jan. 2	18 0	21	29 0

Mean areas and heliographic latitudes of Sun-spots in the year 1896, deduced from photographs taken at the Royal Observatory, Greenwich, at Dehra Dûn (India), and in Mauritius.

(*Communicated by the Astronomer Royal.*)

The results here given are in continuation of those printed in the *Monthly Notices*, vol. lviii. p. 40, and are deduced from the measurements of solar photographs taken at the Royal Observatory, Greenwich, at Dehra Dûn, India, and at the Royal Alfred Observatory, Mauritius.

Table I. gives the mean daily areas of umbræ, whole spots, and faculæ for each synodic rotation of the Sun in 1896 ; and Table II. gives the same particulars for the entire year 1896 and the seven preceding years for the sake of comparison. The areas are given in two forms. First, projected areas—that is to say, as seen and measured on the photographs, these being expressed in millionths of the Sun's apparent disc ; and next, areas as corrected for foreshortening, the areas in this case being expressed in millionths of the Sun's visible hemisphere.

Table III. exhibits for each rotation in 1896 the mean daily area of whole spots, and the mean heliographic latitude of the spotted area for spots north and for spots south of the equator, together with the mean heliographic latitude of the entire spotted area, and the mean distance from the equator of all spots ; and Table IV. gives the same information for the year as a whole, similar results from 1889 to 1895 being added, as in the case of Table II. Tables II. and IV. are thus in continuation of the similar tables for the years 1874 to 1888 on pp. 381 and 382 of vol. xlix. of the *Monthly Notices*.

The rotations in Table I. and Table III. are numbered in continuation of Carrington's series (*Observations of Solar Spots made at Redhill*, by R. C. Carrington, F.R.S.), No. 1 being the rotation commencing 1853 November 9. The assumed prime meridian is that which passed through the ascending node at mean noon on 1854 January 1, and the assumed period of the

Sun's sidereal rotation is 25·38 days. The dates of the commencement of the rotations are given in Greenwich civil time, reckoning from mean midnight.

The principal features of the record for 1896 are :—

(1) The decrease in mean daily spotted area which began in 1894 has continued at an increasing rate.
(2) The decrease in the umbræ is also maintained. As compared with 1895 there is a diminution of 47 per cent.
(3) The faculæ which had shown a revival in 1895 have diminished, and are now less than in 1894.
(4) The decline in spots is far greater in the northern hemisphere than in the southern.
(5) Consequently, the predominance has passed back to the southern hemisphere.
(6) There has again been little change in the distribution of spots in heliographic latitude.
(7) No spots were seen on eight days during the year, and no faculæ on seven days.

TABLE I.

No. of Rotation.	Date of Commencement of each Rotation.	No. of Days on which Photographs were taken.	Mean of Daily Areas. Projected			Corrected for Foreshortening.		
			Umbræ.	Whole Spots.	Faculæ.	Umbræ.	Whole Spots.	Faculæ.
	d							
565	1895 Dec. 23·19	28	166	861	1606	114	631	1812
566	896 Jan. 19·53	26	82	464	841	66	378	966
567	Feb. 15·87	28	205	1413	1187	149	1022	1426
568	Mar. 14·20	26	130	829	1307	93	584	1556
569	Apr. 10·49	28	39	246	935	30	205	1175
570	May 7·73	27	74	433	827	51	301	938
571	June 3·95	27	191	1126	1014	133	783	1167
572	July 1·14	27	128	773	1407	93	576	1553
573	July 28·36	27	53	289	1124	37	209	1228
574	Aug. 24·58	27	273	1656	1703	187	1195	1879
575	Sept. 20·85	27	64	360	1394	48	290	1508
576	Oct. 18·14	28	219	1104	1438	151	794	1568
577	Nov. 14·44	27	93	494	1426	66	363	1603
578	Dec. 11·75	27	198	1130	1348	132	795	1551

TABLE II.

Year.	No. of Days on which Photographs were taken.	Mean of Daily Areas. Projected			Corrected for Foreshortening.		
		Umbræ.	Whole Spots.	Faculæ.	Umbræ.	Whole Spots.	Faculæ.
1889	360	17·9	103	107	13·1	78·0	131
1890	361	21·3	133	273	15·5	99·4	304
1891	363	120	745	1322	86·2	569	1412
1892	362	255	1596	3230	186	1214	3270
1893	362	327	1983	2287	234	1464	2404
1894	364	317	1728	1666	231	1282	1877
1895	364	237	1330	2059	169	974	2278
1896	364	127	745	1243	90	543	1410

TABLE III.

No. of Rotation.	Date of Commencement of each Rotation.	No. of Days on which Photographs were taken.	Spots North of the Equator. Mean of Daily Areas.	Spots North of the Equator. Mean Heliographic Latitude.	Spots South of the Equator. Mean of Daily Areas.	Spots South of the Equator. Mean Heliographic Latitude.	Mean Heliographic Latitude of Entire Spotted Area.	Mean Distance from Equator of all Spots.
	d.			°		°	°	°
565	1895 Dec. 23·19	28	171	13·54	460	12·93	− 5·74	13·09
566	1896 Jan. 19·53	26	99	11·93	278	15·44	− 8·24	14·52
567	Feb. 15·87	28	363	14·85	659	16·00	− 5·04	15·59
568	Mar. 14·20	26	433	15·79	151	15·40	+ 7·71	15·69
569	Apr. 10·49	28	129	15·09	76	13·36	+ 4·55	14·45
570	May 7·73	27	3	17·69	298	11·89	− 11·63	11·94
571	June 3·95	27	137	19·56	646	16·94	− 10·54	17·40
572	July 1·14	27	102	19·18	474	18·48	− 11·80	18·60
573	July 28·36	27	43	12·93	166	17·44	− 11·16	16·51
574	Aug. 24·58	27	835	12·99	359	13·53	+ 5·01	13·15
575	Sept. 20·85	27	89	11·59	201	11·98	− 4·74	11·86
576	Oct. 18·14	28	75	6·14	719	12·25	− 10·51	11·67
577	Nov. 14·44	27	175	11·80	189	13·90	− 1·55	12·89
578	Dec. 11·75	27	167	6·14	628	9·28	− 6·03	8·62

TABLE IV.

Year.	No. of Days on which Photographs were taken.	Spots North of the Equator. Mean of Daily Areas.	Spots North of the Equator. Mean Heliographic Latitude.	Spots South of the Equator. Mean of Daily Areas.	Spots South of the Equator. Mean Heliographic Latitude.	Mean Heliographic Latitude of Entire Spotted Area.	Mean Distance from Equator of all Spots.
1889	360	5·0	+ 7°·26	73·0	−11°·90	−10°·68	11°·61
1890	361	53·1	+22·20	46·3	−21·75	+ 1·73	21·99
1891	363	401	+20·49	169	−19·91	+ 8·52	20·31
1892	362	607	+15·09	607	−21·69	− 3·29	18·39
1893	360	517	+14·91	941	−14·26	− 3·93	14·49
1894	364	543	+12·31	739	−15·56	− 3·75	14·18
1895	364	565	+14·26	409	−12·54	+ 3·01	13·54
1896	364	203	+13·60	340	−14·77	− 4·15	14·33

Notes on the Rotation Period of Venus. By E. M. Antoniadi.

(*Communicated by Capt. W. Noble.*)

The question of the rotation period of *Venus* is one to which the reply has eluded many a distinguished astronomer. Nor is the reason of the failures far to seek. The determination is in itself impossible by ordinary visual methods on account of the formidable diffusion of light by the planet's atmosphere, and until the day arrives when the Doppler-Fizeau principle can be successfully applied to the limb of *Venus*, we may justly display scepticism as to the value of current rotation periods.

The first attempt to determine the rotation period was made by Dominique Cassini in 1666–1667. Cassini considered the spots to be animated by a motion either of libration or rotation, having a period of less than 24 *hours*, round an axis almost coinciding with the plane of the planet's orbit.

Bianchini, who affirms that he had seen *Venus* rotate (1726–1728), fixed the rotation period as 24 *days* 8 *hours*. The axis, he considered, formed an angle of only 15° with the orbital plane.

Influenced by Jacques Cassini's ideas (as shown by Schiaparelli), the fanciful Schrœter found a period of 23 *hours* 20 *minutes* 59·04 *seconds*, while not less biassed must have been Fritsch, in giving, from *four* days' observation, the period as 23 *hours* 22 *minutes*.

Never did scientists, however, indulge more freely in illusions than when the Jesuits of the Roman College undertook, in 1839, under De Vico's direction, the determination of the planet's rotation period. Eleven thousand micrometrical measures of spots taken by Palomba, with unprecedented zeal, gave the value of $23^h\ 21^m\ 21^s{\cdot}9345$.

The honour of demolishing these fanciful results belongs to Professor Schiaparelli, who, in a masterly discussion of all the observations,* concluded that "the rotation of $23^h\ 21^m$ or $23^h\ 22^m$ of Jacques Cassini, Schrœter and De Vico, is the result of a series of paralogisms and vicious circles."

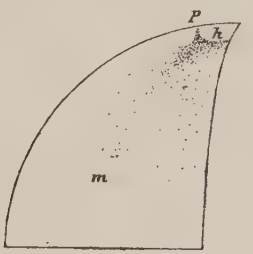


Fig. 1.

Taking as a base the immobility of some spots relatively to the terminator, the illustrious Director of the Milan Observatory

* *L'Astronomie*, 1890.

concluded that *Venus* constantly presents the same face to the Sun, or, in other words, that the rotation period coincided with the 224·7 days revolution round that luminary. Professor Schiaparelli's observations extend from 1877 December 8 to 1878 January 6. The fixed spots forming the ground work of the 224·7 days rotation may be described as follows (Fig. 1) :—

(1) An elongated dusky shading *hm* trending obliquely from the south cusp along the path of a great circle of the dichotomous or crescent phase ;

(2) A bright spot *h* near the cusp, bounded on the left by

(3) A slight diffused shading *p*.

Professor Schiaparelli notes that the spot *h* was seen by Professor Holden on 1877 December 15 ; that something like *h* and *p* was observed by Gruithuisen in 1813 and 1814, and by Vogel and Lohse in 1871.

What strikes us on an examination of these results, is an incredible cloudlessness and wonderful transparency of the atmosphere of *Venus* were these details to belong to the *body* of the planet. Seeing at once this difficulty, Professor Schiaparelli eluded it by stating that the character of these spots " seems to indicate rather atmospherical phenomena ; we consider them analogous to those which form on the surface of the Sun or *Jupiter*, and in default of something better, we will ask permission to similarly use them for determining approximately the rotation period. Cassini and Galileo did not do otherwise when studying the rotation period of *Jupiter* and the Sun."

Leaving at once aside any idea of comparison between the spots on *Jupiter* or the Sun and those of the atmosphere of *Venus*, we may say that we have every reason to believe that the latter can not be far different from the spots of our own gaseous envelope. The spots of our own atmosphere are white clouds floating in the air—a medium strongly diffusing sunlight. (We deem it quite useless to speak of volcanic smoke.) And as it is impossible to imagine clouds appearing black from above, the dusky spot *hm* can scarcely claim an objective existence. If atmospheric, it will at best be a break in a mass of clouds. But if so, is it not singular that this streak takes its position along the path of a great circle of the sphere passing through the cusps of the dichotomous or crescent phase ? Moreover, the atmosphere of *Venus* must be really very calm for a break in the clouds to remain unchanged in position for almost a month. Such constancy of form is somewhat incompatible with what we are witnessing in our aerial envelope, and dwindles into impossibility when we reflect that *Venus* receives twice as much heat as we do from the Sun, and that it is solar heat which sets the air in motion.

White cusp spots like *h* have been repeatedly seen, especially by Trouvelot. But the superior brightness of the limb, to which Sir William Herschel called attention in 1793, can satisfactorily account for them by stopping in the vicinity of the cusps the darkness along the terminator. " With regard to the cause of

this appearance" (the bright limb), says Herschel, "I may venture to ascribe it to the atmosphere of *Venus*, which, like our own, is probably replete with matter that reflects and refracts light in all directions. Therefore, on the border where we have an oblique view of it, there will be in consequence an increase of this luminous appearance."*

We should be somewhat cautious before accepting, as having objective existence, the dusky shading P. For if diffusion of light from the planet's atmosphere hides to our view all markings about the centre of the disc—where, however, the atmospheric effect is reduced to a minimum—we could not reasonably hope to define details almost on the limb itself.

Supposing, however, for one moment, that spots *h* and P are real, then analogies from the planet *Mars* would lead us to consider them as snow caps, *dimmed* in the case of *Venus* by the atmospheric effect. But the presence of snow caps on *Venus* would obviously be the *coup de grace* to the $224^{d}{\cdot}70$ rotation period. The snow on the illuminated hemisphere of a planet presenting always the same face to the Sun would not confine itself to a cap, but would rather distribute itself along the terminator.

It is, then, evident from our reasoning that the foundations of the $224^{d}{\cdot}70$ rotation period of *Venus* are delicate. The markings on which it rests would seem to be contrast effects arising from solar illumination. And we will presently show that fixity of atmospherical shadings with regard to the terminator of an inferior planet does not necessarily imply non-rotation of the body with reference to the Sun

Scarcely had Professor Schiaparelli announced his results, than M. Perrotin, starting from the apparent immobility of a vertical shading with regard to the terminator, concluded that *Venus* rotates in a period comprised between 195 and 225 days.† M. Perrotin's drawings are of very great value. But we should point out here that he confirms Professor Schiaparelli by using a spot which, in spite of its immobility, was rejected by the great Italian himself in his determination of the planet's rotation period. In fact, M. Perrotin's vertical shading is obviously identical with the spot seen by Schrœter in 1788, and of which Professor Schiaparelli said: "The general stability of its form, which lasted for more than two months, its constant parallelism with the terminator, its almost invariable distance from the latter suggest that we have to deal here with some phenomenon of the atmosphere of *Venus*, depending much more on the Sun, and consequently on the terminator circle, than on any axis of rotation whatsoever."‡

In 1895 M. Leo Brenner announced that he had discovered the rotation period of *Venus*, which he fixed, with an accuracy equalled only by De Vico's, at $23^{h}\ 57^{m}\ 7^{s}{\cdot}5459$.

Meantime more than one observer was engaged in confirming

* Paper read before the Royal Society on 1793 June 13.

† *L'Astronomie*, 1890. ‡ *Ibid.* p. 328.

the long rotation. The last observations reach us from Flagstaff.

At variance with all his predecessors and successors, Mr. Percival Lowell finds the markings of *Venus* "as distinct really as those of the Moon. . . . The period of rotation coincides with the period of revolution. *This planet is a desert.*" *

The characteristic feature shown by the Flagstaff observations is a black spot in the centre of the disc from which radiate black anomalous canals, which, to judge from their appearance, would seem to have the intention of extending beyond the limb. Surely such canals, dug as they are in a gaseous envelope, have nothing whatever to do with *Venus*.

For if we know anything certain about this planet it is that it has an atmosphere (in fact, we see it when *Venus* is projected on the vicinity of the Sun), and that this atmosphere is, very probably, denser than that of *Mars*. Now, if diffusion from the rarefied aerial envelope of *Mars* does not allow us to see details about the limb, we could not reasonably hope to see canals on the limb of *Venus*, supposing we are credulous enough to believe for one second in their objective existence.

To believe in the reality of Mr. Lowell's cytherean canals means to believe that the globe of *Venus*, with its atmosphere, is imprisoned in a cage of black hoops meeting about a common diameter directed along the visual ray. Under such circumstances we cannot help considering the whole of this anomalous canal system as entirely illusory, and the central black spot as merely the "pilula" of Fontana.

Last summer I undertook at Juvisy Observatory, under M. Camille Flammarion's direction, a systematic survey of the markings of *Venus*. The observations were made in broad daylight invariably, and the planet was examined at times for three or four hours in succession. No abnormal features were seen at any time. The markings were at all times exceedingly indefinite, and always somewhat doubtful. The impression given by the general appearance of the disc was identical with Sir John Herschel's description, given in his *Outlines of Astronomy* :—"The intense lustre of its illuminated part dazzles the sight and exaggerates every imperfection of the telescope. . . . We notice in it neither mountains nor shadows, but a uniform brightness, in which sometime we may indeed fancy, or perhaps more than fancy, brighter or obscurer portions, but can seldom or never rest fully satisfied of the fact."

The colour of the planet was of a beautiful chrome yellow by day. The limb was exceedingly brilliant ; the terminator of the dichotomised disc dusky, particularly so about its central regions. From each of the cusps a diffused shading entered the disc apparently along the path of a great circle, perpendicular to the orbit of *Venus*, while midway between the terminator and the limb, somewhat nearer the former, an indefinite dusky marking

* *Bulletin de la Société Astronomique de France*, 1897.

extended occasionally from north to south, and almost parallel to the terminator. These various shadings are shown in the annexed Fig. 2.

The cusp markings, which seem to be of the same nature as Professor Schiaparelli's spot *hm* on the contrary phase, were always visible ; but the vertical shading, which is analogous to

Fig. 2.

Schrœter's and M. Perrotin's streaks on the evening star apparition, had a periodical existence only. But when there, all three spots kept constantly the same positions with regard to the terminator, not the slightest trace of rotation being detected.

Obviously such markings are far too symmetrical to induce us to employ them in determining the rotation period.

Examining the causes governing the illumination of *Venus* when dichotomised, we find that such illumination obeys :—

(1) Lambert's law of the cosine.

(2) The increased brilliance towards the limb.

The simultaneous action of these two causes gives a very plausible explanation of the dusky cusp spots, from the fact that the superior luminosity of the border brightens the extremities of the duskiness along the terminator. And as the increase of brightness towards the limb, at first insignificant about the centre, becomes suddenly very marked in the immediate vicinity of the border, the luminous decrease along the terminator will be enhanced by contrast, almost to spots trending obliquely from the cusps along the path of a great circle of the sphere passing through them.

It is less easy to offer a satisfactory explanation of the vertical shading lying midway between the terminator and the limb. But we cannot consider it as having an objective existence ; the more so as the same phenomenon is visible on the evening star phase of *Venus*, and we will not indulge in the wild fancies which would involve cutting the atmosphere of this planet into spherical ungulæ of alternately white and dusky material, having their common diameter at right angles to the orbital plane.

The only reasonable interpretation we can find for this marking is that of an enhanced whiteness about the terminator, such as that of fog particles floating in our morning and evening skies. This view is supported by the fact that the shading in question was not always present.

Everyone knows that *Venus* in quadrature shows not a dichotomous but a crescent phase. No one to my knowledge has found a satisfactory explanation of this phenomenon since Schrœter first called attention to it a hundred years ago. An attempt at an explanation was recently given by me,* namely, that it was due to the superior brightness of the limb, when the cusps would stand out "in noble relief, *through contrast and irradiation*, from the sombre central regions of the terminator." This is certainly so ; but the interpretation given is not quite complete, and I am indebted for its full development to my friend, Abbé Moreux, whose theory may be summarised as follows :—

Let ABO (Fig. 3) stand for a quadrant of dichotomised *Venus*, OB the direction of the Sun, and CDEF the planet's atmosphere. The atmospheric illumination does not stop at DO but at CG. Towards OG we are looking on less atmospheric particles than at

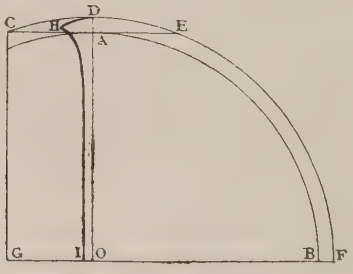


Fig. 3.

ADC where the luminosity is very considerably increased. Hence the final terminator curve will take a path such as DHI with a preponderance of brilliance at the exaggerated cusp H.

Owing to the great analogy of dimensions, we are fully justified in considering *Venus* as being the planet the most resembling our Earth. Now observation shows that the heights of

* *Journal of the British Astronomical Association*, vol. viii. No. 1.

the atmospheres of the two planets are also comparable, since that of *Venus* would measure, according to D'Abbadie, some 250 miles (2″ on a diameter of 62″·8 during the 1882 transit on the Sun), while that of the Earth extends from 190 to 212 miles, according to M. Liais, from observations on the influence of the highest regions of our atmosphere upon twilight at Rio Janeiro.* Here I think the comparison is not quite in favour of the height of the Earth's atmosphere, for if the excellent background of the corona cannot fail to show the slightest particles of the most rarefied regions of the atmosphere of *Venus*, we could not speak as confidently of the value of atmospheric heights based on twilight observations.

What the density of the atmosphere of *Venus* is we do not know, for we cannot rely on the discordant values of horizontal refraction (Schrœter 30′ 34″ ; Mædler 43′·7.)† It is probably not far different from our own. *Venus* receiving from the Sun an amount of heat per unit of area which is to what we enjoy as $\left(\frac{1}{\cdot 723}\right)^2 = \frac{1\cdot 91}{1}$, evaporation must be more active on this planet than on the Earth. But we should hesitate in assuming that the cytherean skies are usually overcast, for the reason that if the greater heat stimulates evaporation, the converse process of precipitation would be slackened for the same reason.

Applying to *Venus* Professor Langley's investigations on terrestrial atmospheric absorption, we should have the normally falling ray losing 40 per cent. of its intensity in its passage downward through the atmosphere. Of the 60 per cent. reaching the planet's surface less than one-quarter would be diffused by yellow sand itself, that is, 14 to 15 per cent.

And this quantity must lose again 40 per cent. in its return passage through the gaseous envelope. Thus no more than $\frac{1}{12}$th of the luminous rays ‡ would reach the eye of an observer placed in space. In spite of its apparent exiguity, this $\frac{1}{12}$th of the total radiation which we would see about the centre of the disc viewed along the radius vector, is a very considerable quantity, and one which would doubtless allow us to have a tolerably fair idea of the distribution of light and dark spots over the planet's surface, were it not for the supreme blurring cause—luminous diffusion from the planet's atmospheric particles. The effect of this diffusion is to reduce the contrast between the light and dark areas, to the utter blurring of the image.

Beneath its dazzling gaseous envelope, showing no more than some fugitive markings—at best functions of the phase—rotates the invisible globe of *Venus* in a period which is quite unknown. Terrestrial and Martian analogies, however, as well as the existence of some sort of relation between the masses of the planets and their rotation periods, would give to *Venus* a diurnal

* Moore's *Meteorology*, London, 1894, pp. 16–17.
† Chambers's *Descriptive Astronomy*, 3rd edition, p. 68.
‡ Flammarion, *La Planète Mars*, p. 89.

rotation comparable to that of the Earth or *Mars*—a view supported by the great probability that the extraordinary results claimed by the long rotation could not reasonably have been brought about by tidal friction.

Long-enduring Spots on Jupiter. By A. Stanley Williams.

There are several cases on record of spots on *Jupiter* which have remained in existence for periods measured by a number of years. The first authentic instance of the kind relates to the celebrated spot which appeared in the latter half of the seventeenth century, and which may possibly be identical with the present well-known "red spot." This last-named object has been under constant observation for at least twenty years, whilst its existence can be traced for perhaps thirty or forty years more. A third instance is that of the brilliant white equatorial spot which attracted so much attention in 1880, and remained visible for at least five or six years. Such cases of extreme longevity are not only interesting in themselves, but are of extreme importance in connection with questions relating to the present physical condition of *Jupiter*.

The object of the present paper is to put on record another instance of remarkable longevity in a Jovian spot. The object in question was a brilliant white spot situated on the north edge of the great north equatorial belt. For convenience it is designated C. The following is a complete list of all the observed times of transit of this spot which have come to the writer's notice,* together with the corresponding longitudes, all uniformly expressed according to the "System II." of the late Mr. Marth's ephemerides for 1887 and 1888.† The zero meridian of this system has several times been subsequently shifted 10°, in order to make it correspond nearly with the centre of the red spot. The longitudes given here refer to the unshifted zero meridian. For the observations made by the writer, the weights attributed to them at the time have been added. These weights range from 1 (bad) to 5 (good). An additional column gives the brightness of the spot according to a uniform scale, in which eeB=most exceedingly bright, eB=exceedingly bright, vB=very bright, B=bright, mB=moderately bright, F=faint, vF=very faint, eF=exceedingly faint. The total number of transits is 153.

* The spot also appears in some of the Lick photographs of *Jupiter*, but it has not been thought necessary to measure these, the visual observations being already sufficiently numerous.

† The daily rate of rotation of this system is 870°·2700, corresponding to a period of rotation of $9^h\ 55^m\ 40^s{\cdot}63$.

Observations of Bright Spot C.

Date.	G.M.T. of Transit. h m	Weight.	Longitude. °	Brightness.	Observer.
1885.					
Jan. 27	11 1	...	260·1	...	Denning.
Mar. 4	10 21	...	250·3	...	,,
7	7 54	...	252·6	...	,,
11	11 4	...	248·8	...	,,
12	6 57	...	249·9	...	,,
14	8 29	...	246·1	...	,,
19	7 50·3	...	(254·4)	...	Terby.
28	10 3	...	247·3	...	Denning.
31	7 32·8	...	247·3	...	Terby.
Apr. 2	9 8	...	245·4	...	Denning.
4	10 45	...	244·5	...	,,
12	7 19	...	241·9	...	,,
18	12 20	...	245·1	...	,,
19	8 7	...	242·4	...	,,
19	8 11·7	...	245·3	...	Terby.
20	13 49·7	...	239·7	...	Barnard.
23	11 23	...	241·6	...	Denning.
25	12 55·2	...	237·7	...	Barnard.
May 1	8 0	...	240 3	...	Denning.
June 1	8 33	...	234·0	...	,,
3	10 18	...	237·6	...	,,
13	8 19	...	226·4	...	,,
20	9 11	...	228·2	...	,,
1886.					
Apr. 6	7 27	...	162·4	...	,,
13	8 14	...	162·6	...	,,
25	8 4	...	160·9	...	,,
27	9 42	...	160·7	...	,,
30	7 13	...	161·5	...	,,
1887.					
Feb. 26	15 45	3	93·2	B	Williams.
Apr. 8	14 9	3	81·1	,,	,,
16	10 46	est.	81·7	vB	,,
20	13 51	3	75·1	B	,,
25	13 0	3	76·2	vB	,,
30	12 6	5	75·5	eB	,,
May 10	10 19·5	4	74·8	,,	,,
14	13 39	3	76·7	vB	,,

Date.	G.M.T. of Transit.	Weight.	Longitude.	Brightness.	Observer.
1887.	h m		°		
June 8	9 10·5	...	71·7	...	Terby.
10	10 45	3	69·3	B	Williams.
10	10 45·5	...	69·6	...	Terby.
15	10 0	est.	73·3	B	Williams.
17	11 36	4	71·7	,,	,,
20	9 5·5	...	71·4	...	Terby.
22	10 36	3	66·4	B	Williams.
22	10 38·5	...	67·9	...	Terby.
25	8 16·5	...	72·7	...	,,
27	9 48·5	...	68·6	...	,,
July 2	8 57·5	...	68·7	...	,,
14	8 51·5	...	66·8	...	,,
26	8 42·5	...	62·7	...	,,
1888.					
Mar. 25	14 7	3	19·7	vB	Williams.
Apr. 1	14 56	1	21·8	B	,,
3	16 33	3	21·2	vB	,,
6	13 53·5	4	15·8	,,	,,
13	14 41·7	4	17·6	eB	,,
20	15 27	5	17·7	eeB	,,
May 3	11 4	3	14·0	vB	,,
10	11 57	4	18·9	,,	,,
12	13 30	3	15·9	,,	,,
14	15 9	3	16·5	,,	,,
15	11 11	2	23·1	B	,,
19	14 17·5	3	18·0	eB	,,
20	10 10·5	3	18·5	vB	,,
24	13 25	1	17·6	,,	,,
27	10 48	3	14·0	,,	,,
June 1	9 59	4	16·3	eB	,,
3	11 38	2	16·8	vB	,,
10	12 20·5	2	15·0	,,	,,
13	9 49	5	14·5	,,	,,
22	12 13	1	14·4	B	,,
July 19	9 30	1	12·8	,,	,,
Aug. 22	7 45	est.	14·6	vB	,,
1889.					
Mar. 2	17 10	1	11·1	,,	,,
26	17 55	1	14·1	F	,,

Date.	G.M.T. of Transit.	Weight.	Longitude.	Brightness.	Observer.
1889.	h m		°		
Apr. 22	14 42	1	25·4	F	Williams.
July 4	9 56·5	3	30·9	mB	,,
6	11 29·1	2	27·6	,,	,,
21	8 43·8	2	22·9	,,	,,
30	11 8·7	1	23·2	vF	,,
Aug. 26	8 26·7	est.	21·8	,,	,,
Sept. 12	7 45	...	29·3	...	Denning.
1890.					
May 20	14 59 6	2	18·6	mB	Williams.
27	15 46·1	1	18·9	,,	,,
30	13 15	1	18·6	B	,,
July 3	11 13·3	2	17·6	,,	,,
22	11 58·2	1	22·5	vF	,,
24	13 30·4	2	19·0	F	,,
Aug. 1	10 4·5	2	17·7	mB	,,
3	11 49 8	3	22·2	,,	,,
5	12 30·2	2	23·7	,,	,,
8	10 51	1	18·6	,,	,,
15	11 37·6	3	19·3	,,	,,
30	9 0·4	1	19·3	,,	,,
Sept. 4	8 12·5	2	21·8	,,	,,
6	9 47·8	2	20·0	F	,,
8	11 21·9	2	17·4	,,	,,
16	8 3·5	3	19·5	,,	,,
Oct. 10	7 53	2	17·8	mB	,,
17	8 44·6	1	20·0	,,	,,
Nov. 1	6 10·1	2	18·2	F	,,
Dec. 7	6 3·3	1	16·0	...	,,
1891.					
June 17	15 23·3	3	13·7	mB	,,
July 4	14 22·1	2	11·9	,,	,,
Aug. 7	12 13·8	...	6·7	,,	,,
12	11 19·4	3	6·4	,,	,,
16	14 35·3	4	5·7	,,	,,
19	12 4·9	4	6·0	B	,,
28	14 22·2	4	2·7	mB	,,
29	10 16·8	3	4·8	,,	,,
Sept. 5	10 57	4	1·9	,,	,,
9	14 6·1	2	357·8	F	,,

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Date.	G.M.T. of Transit.	Weight.	Longitude.	Brightness.	Observer.
	h m		°		
1891. Sept. 10	9 56	2	357·0	F	Williams.
12	11 35·7	2	358·1	mB	,,
22	9 40·5	est.	(352·2)	B	,,
27	8 57·6	3	358·0	mB	,,
Oct. 4	9 39·7	3	355·7	vB	,,
28	9 28	2	354·9	mB	,,
30	11 10·5	2	357·3	F	,,
Dec. 20	8 39·7	1	2·4	mB	,,
1892. July 7	14 35·3	3	352·4	,,	,,
Aug. 20	11 11	est.	(1·7)	,,	,,
21	16 48·8	2	356·3	F	,,
22	12 42·0	3	357·4	,,	,,
26	16 3·9	est.	358·5	,,	,,
31	14 52·9	4	349·7	mB	,,
Sept. 8	11 38·3	3	355·3	,,	,,
10	13 13	2	353·3	F	,,
15	12 17	3	351·4	vF	,,
17	13 55·8	2	351·9	F	,,
18	9 56	1	357·4	,,	,,
24	14 35·3	3	348·6	,,	,,
Oct. 2	10 59·8	1	341·7	vF	,,
6	14 12	2	339·5	,,	,,
7	10 1·1	3	338·2	F	,,
9	12 1·4	2	(351·7)	,,	,,
11	13 19·5	3	339·7	vF	,,
12	9 13·1	2	341·2	,,	,,
23	13 15	2	341·7	,,	,,
Nov. 7	10 27	3	335·4	,,	,,
24	9 34·8	3	338·8	,,	,,
27	7 4·2	2	338·5	,,	,,
Dec. 4	7 51·4	1	338·6	vF?	,,
6	9 30·4	2	338·8	eF	,,
11	8 43·9	2	341·8	,,	,
13	10 21·8	est.	341·2	...	,,
21	6 53·8	est.	336·8	eF	,,
23	8 35·3	2	338·4	vF	,,
1893. Aug. 14	12 53·5	2	332·8	mB	,,
16	14 24·2	1	(328·1)	...	,,

Date. 1893.	G.M.T. of Transit. h m	Weight.	Longitude. °	Brightness.	Observer.
Aug. 23	15 25·9	1	337·1	F	Williams.
28	14 31	est.	335·3	F?	,,
Sept. 4	15 12	1	332·1	eF	,,
Oct. 8	13 3·3	3	325·7	F	,,
Nov. 6	12 4·5	1	331·6	vF	,,

It will be seen from the foregoing list that the spot was under observation from 1885 January 27 to 1893 November 6, a period of 3205 days, or nearly nine years. Such a long duration is the more remarkable when it is considered that the spot was of comparatively small dimensions, its diameter, in fact, not much exceeding that of Satellite III.

The variations in the rate of motion of the spot are no less remarkable than its longevity. It is a very significant circumstance that none of these long-enduring spots have preserved a uniform motion. The great red spot even, it is well known, has varied considerably in its rate of motion from time to time. The present spot C is no exception to this rule. The following table gives its approximate period of rotation for each opposition, together with the number of rotations comprised between the first and last observation. The observations of 1886 are insufficient to give more than an approximate result, but the position of the spot in that year relative to that which it occupied in 1885 and in 1887 shows that its real rate of rotation in the opposition of 1886 probably differed little from what it was in the other two years :—

Opposition.	Rotation Period. h m s	No. of Rotations.	Opposition.	Rotation Period. h m s	No. of Rotations.
1885	9 55 32·3	348	1890	9 55 40·3	485
1886	(36·2)	58	1891	36·2	449
1887	32·	6280	1892	35·7	408
1888	38·8	362	1893	35·8	203
1889	43·7	468			

The following is a short history of the motion of the spot gathered from the foregoing table in conjunction with the previous list of observations. From 1885 January 27 to 1887 July 26 the motion was almost perfectly uniform, the rotation being performed in a period of $9^h\ 55^m\ 32^s{\cdot}5$. This rate of motion seems to have been continued almost up to the commencement of the observations of 1888, when a sudden check occurred, and the spot came nearly to a standstill with respect to Marth's zero meridian. This state of things continued almost unaltered all through the opposition of this year. In 1889 March we still find the spot in nearly the same longitude, but its motion then became still slower, so that the mean period of rota-

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tion for the apparition was as long as $9^h\ 55^m\ 43^s{\cdot}7$. A slight increase, however, occurred in the velocity of the spot in 1890, whilst in 1891 a still further increase in velocity took place, the period of rotation diminishing to $9^h\ 55^m\ 36^s{\cdot}2$; and this accelerated rate of motion was maintained practically unaltered, though with temporary slight variations, in the two succeeding years. The spot was unfortunately lost sight of after 1893 November 6, owing to the enormous changes which were occurring in that year in the region of the north equatorial belt, changes which quite altered the aspect of this part of the planet in the course of a few months.

In connection with the probable cause of the arrestment of the spot's motion in 1888, it is necessary to consider the conditions prevailing along the whole zone in which it was situated. In 1887 a somewhat abnormal condition of affairs had prevailed along this zone. Whilst in one hemisphere of *Jupiter* the spots were rotating at an average rate of $9^h\ 55^m\ 41^s{\cdot}3$, in the other hemisphere the spots—amongst them our spot C—were rotating in $9^h\ 55^m\ 32^s{\cdot}2$.* It is obvious that such a state of things could not endure permanently if the two series of spots were at the same level, but that ultimately the swifter spots must overtake and collide with the more sluggish ones. This is exactly what appears to have happened, with the result that the motion of the swifter spots was arrested, and made to conform to that of the more sluggish ones. The velocity of the material of the zone consequently became nearly uniform right round the planet. Subsequently to 1889 the acceleration in velocity of spot C that then occurred seems to have affected more or less the whole zone.

An interesting point about the arrestment of the velocity of spot C is that it was not perfectly regular. There were obviously slight minor fluctuations from time to time in the position of the spot. Such fluctuations might well be expected to occur in the case of two opposed masses of material, possessed of differing velocities, coming into a state of equilibrium.

A few words should be said relative to the appearance of the spot. When at its greatest intensity it was a very brilliant object, appearing as a definite disc, slightly oval in shape, and rather larger than satellite III. But at other times, even when brilliant, it appeared quite indefinite, whilst on several occasions feebler extensions were seen in an east or west direction. It is probable that such variations in aspect are answerable for some of the discordances in the observations.

The fluctuations in the brightness of the spot, though considerable, were not nearly so great or so sudden as were those experienced by the white equatorial spot of 1880. It is probable, too, that some of the differences here recorded had an atmo-

* Further particulars concerning this anomalous state of things will be found in the writer's *Zenographical Fragments*, p. 18.

spheric origin. The visibility of very large spots, provided that they are not faint, is almost independent of the state of the seeing. But the smaller a spot may happen to be, the more is its visibility affected by poor definition. Such a spot as C would not be altogether obliterated by the worst and most confused seeing obtainable in practice, but it would certainly appear less conspicuous under such conditions.

Equatorial Comparisons of Neptune with 114 (o) *Tauri*, 1897 *December*. By John Tebbutt.

In consequence of illness and cloudy weather I found it impossible to obtain earlier comparisons than those which accompany this letter. The observing conditions were, however, excellent on each evening, and the measures were made with the filar micrometer on the 8-inch equatorial and in a bright field. Each coordinate of the planet is the result of twenty comparisons. The mean place of the star for 1897·0 is derived from the following Catalogues: Washington, 1860, 3rd ed.; Radcliffe, 1860–1890; Glasgow, 1870; and Greenwich, 1864, 1872, 1880. The precessions with the secular variations have been employed from the Radcliffe Catalogue, 1890, with checks from the same elements in the Greenwich Catalogue, 1880, and the annual proper motions in R.A. and N.P.D. have been adopted as $-0^{s}\cdot0011$ and $-0''\cdot006$. By assigning equal weights to the Catalogues the mean place for 1897·0 is R.A.$=5^{h}\ 21^{m}\ 26^{s}\cdot88$ N.P.D.$=68°\ 9'\ 4''\cdot3$. The star is also one of Professor Newcomb's Standard Clock and Zodiacal stars.

In the last column of the accompanying table will be found a comparison of the observed places with the transit ephemeris on page 281 of the *Nautical Almanac*. The *Nautical Almanac* does not furnish any semidiameter for the planet, but the *American Ephemeris* assigns $1''\cdot3$ for the time of opposition. This value must, I think, be much too great.

Results of Micrometer Comparisons of Neptune and 114 (*o*) *Tauri.*

1897.	Windsor Mean Time.	Planet Centre—Star. R.A.	Planet Centre—Star. N.P.D.	Star Reductions. R.A.	Star Reductions. N.P.D.	Parallax Corrections. R.A.	Parallax Corrections. N.P.D.	Geocentric Apparent Place of Planet's Centre. R.A.	Geocentric Apparent Place of Planet's Centre. N.P.D.	Correction to *Nautical Almanac.* R.A.	Correction to *Nautical Almanac.* N.P.D.
	h m s	m s	′ ″	s	″	s	″	h m s	° ′ ″	s	″
Dec. 22	10 55 18	−0 53·06	+6 26·5	+6·02	−9·4	0·00	+0·2	5 20 39·84	68 15 21·6	−0·12	+3·1
„ 23	10 33 33	−1 0·16	+6 32·2	+6·03	−9·4	0·00	+0·2	5 20 32·75	68 15 27·3	−0·15	+3·1
„ 24	10 9 27	−1 7·15	+6 37·9	+6·03	−9·4	0·00	+0·2	5 20 25·76	68 15 33·0	−0·11	+3·1
„ 26	10 10 5	−1 21·32	+6 48·7	+6·05	−9·3	0·00	+0·2	5 20 11·61	68 15 43·9	−0·12	+2·8
„ 27	9 59 10	−1 28·27	+6 54·4	+6·05	−9·3	0·00	+0·2	5 20 4·66	68 15 49·6	−0·09	+3·0
„ 28	10 19 24	−1 35·40	+6 59·9	+6·06	−9·3	0·00	+0·2	5 19 57·54	68 15 55·1	−0·14	+3·0

Windsor, New South Wales:
1898 *January* 23.

Nebulæ observed at the Royal Observatory, Cape of Good Hope.

(*Communicated by Dr. David Gill, C.B., &c., H.M. Astronomer.*)

Name.	R.A. 1860. h m s	Dec. ° ′	Authority for position.	Description.	Observer.
N.G.C. 1398, Winnecke	3 32 58·2	−26 47·9	Equ. comp.	Small round neb. Bright in middle	F. 1887 Mar. 17
C.P.D. −47°, 418 New	4 7 34·3	−47 37·3	C.P.D.	Nebulous star (8·8 vis., 10·4 photo.), nebulosity 1′ in diameter	I. 1897 Feb. 5
New	11 34	−45 42	Circle reading	...	F. 1884 July 27
New	14 14	− 5 21	Circle reading	...	F. 1883 Sept. 20
Cor. D.M. −35°, 9764 New	14 36 45	−35 34·7	Cor. D.M.	Elliptical neb. surrounding two stars as if they were the foci of an eclipse, mags. 9·5 and 10. The Cor. D.M. mag. of the chief star is 9·7. In a high-power field with Lac. 6076	I. 1897 Feb.
C.P.D. −32°, 3780 N.G.C. 5824, Barnard	14 55 22·7	−32 30·8	C.P.D.	C.P.D. mag. = 9·4. Bright small nebula, mag. 9·0	F. 1883 Sept. 25
New	16 23	−25 45	Circle reading	Follows a faint star 4·5 secs., and is 0′·5 S.	F. 1887 Sept. 8
N.G.C. 6302, Barnard	17 4 17·5	−36 55·8	Equ. comp.	Small, very bright nebula	F. 1887 Sept. 11
New	19 20 36	−46 59	Circle reading	A faint nebula joined to, but *np*, a 9·5 mag. star. There is perhaps a stellar nucleus.	I. 1897 Nov. 11

Name.	R.A. 1860. h m s	Dec. ° ′	Authority for position.	Description.		Observer.
Brit. Ast. Assn. Journal, vol. vi. p. 257, C.P.D. −46°, 9730	19 23 23	−46 11·2	C.P.D.	C.P.D. mag. = 9·4, an even patch of light 3″ by 2″	I.	1895 Oct. 16
New	20 55	−56 48	Circle reading	Equal to a 9·7 mag. star, elongated 15″; perhaps a small group of stars or a ring nebula	I.	1897 Nov. 26
Swift, 1897 Aug. 8.	22 33 30	−45 31	Circle reading	...	F.	1886 Dec. 26
New	22 49	−44 8	Circle reading	...	F.	1886 Dec. 25
N.G.C. Apx. 1459, Barnard	22 49 20·1	−37 12·6	Equ. comp.	Differs from Dreyer 10 secs. and 2	F.	1886 Dec. 16
G.C. 4992, *h* 4000	23 28 45	−38 43·2	Alignment	Between Cor. D.M. −38°, 15559 mag. 9·7 and −38°, 15561 mag. 9·9, near the former. Impression = 11th mag. The coordinates in the N.G.C. are 30′ ± in error	I.	1897 Dec. 18

Observers: F., W. H. Finlay, 7-inch and 6-inch equatorials; I., R. T. A. Innes, 7-inch equatorial.

Royal Observatory, Cape of Good Hope:
1898 *January* 21.

List No. 6 of Nebulæ discovered at the Lowe Observatory, Echo Mountain, California.* By Lewis Swift.

No.	Date. 1897	R.A. h m s	Dec. ° ′ ″	Descriptions.
1	Dec. 25	2 11 10	−31 39 15	pF, pS, R, distant D ✶ nf.
2	22	2 25 45	−34 42 0	eeeF, S, R, D ✶ nearly p, np of 2.
3	22	2 26 0	−34 41 42	eF, eS, R, F ✶ nr n.D ✶ np, sf of 2.
4	22	2 26 15	−36 28 55	pB, pS, vE.
5	22	2 37 0	−28 37 0	eeF, S, R, 3 D stars nf, each about 7″.
6	22	2 40 35	−28 22 35	eeF, S, R, D ✶ np.
7	22	3 7 30	−25 42 0	eeF, S, R, 2 f stars near sp point to it.
8	26	3 35 40	−27 11 10	eF, eS, lE, ✶ close nf, stellar.
9	26	3 50 40	−28 26 15	eF, S, R, F ✶ in contact nf.
10	23	4 8 30	−32 49 48	vF, v, S, R, partial resolvability suspected. 1531-2 in field.
11	26	4 52 0	−28 41 35	eeeF, pL, iR, D ✶ 24ˢ f point to it.
12	Nov. 30	5 2 30	−20 37 15	eeeF, pS, bet 2 stars, eeedif. Close to eeeF D ✶
13	Dec. 26	5 15 10	−25 11 30	eeeF, vS, R, 7ᵐ ✶ 15ˢ p. ls nearly obliterates it eeedif.
14	1	5 30 0	−23 36 30	eeF, pS, R, 7ᵐ ✶ close f, sf of 1980.
15	Nov. 30	5 44 0	−30 31 55	eeeF, pS, R, F ✶ np, sev B st sf, 3 st n curved.
16	Dec. 1	5 53 0	−23 11 30	pB, pS, R, in vacancy, sev B st f.
17	1	5 56 45	−23 41 30	B, L, R, bet a ✶ nf and v wide D ✶ np.
18	26	6 1 5	−27 51 50	vF, pS, lE, ✶ in contact nf, n end like a brush.
19	28	9 18 10	−32 2 55	pF, eS, vE, 10ᵐ ✶ close sp.
20	28	9 54 5	−26 42 35	eeeF, eeS, R, eF D ✶ close S, eeedif, 3078 in field.
21	28	10 11 20	−33 3 10	eeF, pS, iR, in centre of trapezium.
22	30	10 16 38	−33 46 15	vF, eS, R, 9ᵐ ✶ p close f.
23	30	10 24 30	−35 3 15	eeeF, eeS, R, eF, ✶ in contact S, p of 2.
24	30	10 35 12	−35 31 35	eeeF, eeS, R, eF, ✶ in contact S, f of 2.
25	29	10 36 0	−35 5 25	eeeF, eS, R, eeF D ✶ near S.

Notes to numbers 23 and 24.

Here are two nebulæ singularly placed whose descriptions, as will be seen, are identical in every particular. I ran across a very faint nebula which I found was N.G.C. No. 3267. Near following was an exceedingly faint and very close double star, which, with a power of 132, looked as if the north one was

* Lists Nos 2, 3 and 4 will be found in *Monthly Notices*, vol. lvii. pp. 629, 631, and vol. lviii. p. 18; Lists Nos. 1 and 5 in the *Astronomical Journal*, Nos. 388, p. 27, and 422, p. 111.

an exceedingly small nebula of unimagined faintness. With a power of 200 my suspicion that it was not a star was confirmed.

In a few minutes I ran across another, which in every particular was exactly like it, both being north of its companion star, and of the same distance apart about 4′, and of the same size and faintness. But for the excellent seeing and superiority of my periscopic eyepiece for revealing faint nebulæ, they would have escaped detection. If they are as distant as their companion stars, they must vastly exceed in volume the orbit of Neptune, and yet are self-luminous.

List No. 7 of Nebulæ discovered at the Lowe Observatory, Echo Mountain, California. By Lewis Swift.

No.	Date. 1898.	R. A. 1900·0. h m s	Dec. ° ′ ″	Description.
1	Jan. 22	9 23 0	−42 24 30	pB, S, R, 7^m * nf, D * p.
2	1897 Dec. 30	9 45 0	−32 23 5	pB, pS. eeE, spindle, 7^m * np, np of 2. Not 3038.
	30	9 45 35	−32 25 5	eeF, eS, 3F st close f, sf of 2.
4	30	9 52 32	−31 48 0	vF, S, R, $7\frac{1}{2}^m$ * np, 2 or 3 Fst near.
5	30	10 24 30	−35 3 15	eeeF, eeS, R, eF * in contact sp of 2.
6	1898 Jan. 1	10 26 3	−28 12 30	pF, vS, R, trapezium near sp.
7	1	10 27 10	−29 52 35	eeeF, pL, R, D * nr sf, * with dist comp. f and p.
8	1897. Dec. 30	10 35 12	−35 31 35	eeeF, eeS, R, eF * in contact sf of 2.
9	1898. Jan. 1	11 17 0	−28 27 30	pB, pS, R, 10^m * close n little f, 7^m * f.
10	1897. Dec. 29	11 49 35	−37 21 5	eF, vS, 7^m * sp.
11	30	12 0 30	−27 22 45	vF, L, lE, 8^m * nr f, np of 2.
12	1898. Jan. 1	12 0 35	−27 24 25	eeeF, pL, eE, 3 8^m st f, sf of 2.
13	31	12 3 25	−31 2 15	pB, vS, * close sf, vE 45°.
14	1	12 14 28	−25 37 15	pB, S, R, bet 4 st sf and 8^m * np.
15	30	12 18 45	−39 14 0	pF, vS, R, close p of 4373. See note.
16	1	12 20 0	−25 30 15	eF, vS, R, bet 7^m * sf and 8^m * np, v dif.
17	30	12 22 5	−38 48 50	pB, pL, R, 7^m * with dist comp. near p.
18	1	12 45 0	−25 22 15	eeeF, S, R, 8^m * nf.
19	31	12 46 0	−29 27 35	B, S, lE, 9^m * near sf.
20	31	13 11 10	−31 33 45	pB, pS, R.
21	31	13 12 5	−31 7 45	eeeF, pL, R, 9^m * nr sp, e dif.
22	31	13 18 25	−29 47 37	eeF, pS, R, trapezium nr sf.
23	30	13 31 0	−33 35 0	pL, pS, lbM.
24	30	13 31 10	−33 33 55	eeeF, eeS, like D *, one nebulous. Note.
25	30	13 51 35	−39 31 50	8^m * in eeeF nebulosity. See note.

Notes.

No. 15 forms D nebula with 4373. Strange *h* failed to see it.

No. 24. This is another of that variety resembling a close D ✶, making four I have lately found, dis. about 4″. That they are physically connected is highly probable.

No. 25. This is a nebulous star, the only one I have ever found. The central star is about 8ᵐ, and surrounded with an exceedingly faint atmosphere. An 8ᵐ star follows 15ˢ, which was free from nebulosity.

Some of the above list are quite interesting objects, especially for their brightness and size, and their remaining so long undiscovered. This list makes the number of nebulæ discovered at this Observatory 205. I find the southern sky visible from this Observatory an interesting field for the search of nebulæ.

List No. 8 of Nebulæ discovered at the Lowe Observatory, Echo Mountain, California, for 1900·0. By Lewis Swift.

No.	Date. 1898.		R.A. h m s	Dec. ° ′ ″	Description.
1	Feb.	22	5 29 20	−36 28 30	eeeF, eeS, eeeE, eee dif. See note.
2		14	5 30 8	−23 24 15	eF, vS, R, 8ᵐ ✶ f 10ˢ in field with 1979.
3	Jan.	31	5 47 40	−38 22 45	eeeF, S, cE, semi circle of 3 st, S, v dif.
4	Feb.	20	9 31 5	−11 56 30	pB, pL, R, 2 st near f.
5		19	9 39 40	−31 18 35	eF, S, R, vF ✶ close nf, pB ✶ near sp.
6	Jan.	12	9 45 5	−32 27 40	eeeF, pS, vE, bet below ✶ and 8ᵐ ✶ p, nf of 2.
7		12	9 45 30	−32 27 45	eeeF, eeeS, R, D ✶ close sf, sp, of 2.
8	Feb.	19	9 52 40	−31 47 40	vF, pS, R, 8ᵐ ✶ p.
9		15	9 55 25	−29 10 30	eeeF, pS, cE, trapezium n and nf, D ✶ np.
10		14	9 59 25	−27 4 50	eeF, L, cE, no B ✶ nr, no Δ as per 3113.
11		15	10 27 30	−23 32 10	eeeF, eS, eE, 8ᵐ ✶ close p, eee dif.
12		15	10 31 30	−23 34 10	eF, pS, R, bet 2 D st sp and nf.
13		22	10 32 35	−10 35 55	CB, eS, Stellar.
14		14	10 33 25	−26 32 20	pF, pS, D ✶ nr p. See note.
15	Jan.	14	10 58 30	−15 41 45	eeF, eeS, looks like a D ✶.
16	Feb.	12	11 26 50	−29 52 40	vF, pS, R, sf of 3717.
17		22	11 44 0	−11 45 40	eF, cL, iR, Δ with ✶ n and another f.
18		20	11 45 25	−19 1 57	B, S, eE, a ray.
19		15	12 14 10	−25 35 20	pF, vS, R, 8ᵐ ✶ nr np.
20		15	12 35 15	−36 13 20	pF, vS, 2 or 3 vF st in contact.
21		23	12 44 20	− 3 51 25	eeF, L, eE, 7ᵐ ✶ nr s little f.
22	Jan.	31	12 55 0	−31 43 50	eeF, pS, R, 10ᵐ ✶ nr nf.
23		30	13 59 40	−38 43 40	eeF, pS, R, bet 2 st, nr centre of trapezium.
24	Feb.	22	14 6 50	−30 3 33	F, pS, R, surrounded with sev F st.
25		22	14 28 20	−27 7 18	pB, eS, R, looks like a D ✶ one nebulous.

Notes.

N.G.C. 3145. I make the place of this interesting nebula for 1860 10^h 3^m 3^s $-11°$ 38′ 45″ vB, L, vE, much obscured by proximity to λ Hydræ np. New Gen. Catalogue has 10^h 3^m 18^s $-11°$ 44′ 18″, F. pL, R. It is very strange H. should say nothing about the star. As, however, our places are not very wide apart, I assume that my object is identical with 3145, yet that he should call it round is another mystery. It is not permissible to suppose that since its discovery in 1786 any change has taken place.

No. 1. This list is another hair line nebula, much resembling No. 7, list 4. There is a slight bulging in the centre, but it requires very close scrutiny to see it. They must be rings or flat disks placed parallel to our line of sight. In the field preceding there are several stars forming a segment of a large circle, and 3 stars like belt of *Orion*. No bright star near.

No. 14. This is not one of Sir John Herschel's 9. I have another near; stellar.

No. 19. List No. 6, as published in A.J. and perhaps other publications, contains a typographical error. For Dec $-22°$ read 32°.

A Remarkable Object in Perseus. By the Rev. T. E. Espin, B.A.

On the night of January 16, while sweeping for red stars and stars with remarkable spectra, I passed suddenly from the starry background into what appeared to be a cloud. Although the night was very clear, yet I felt convinced it was a cloud, and continued my sweep. At the end of the sweep the telescope, as is usual, was moved 40′ south and the return sweep made. I again came upon the peculiar obscuration. My suspicions were aroused, as it seemed strange that on a clear sky there should be a small cloud, and that stationary. I waited twenty minutes, and then re-examined the object, and found it still there. I marked it on the B. D. charts, and next morning turned to the New General Catalogue of Nebulæ and the Addenda, but could not find it. On January 24 I re-examined it, and by rapid sweeping laid down its limits. They are as follows :—

In R.A. from 4^h 23^m 30^s to 4^h 28^m 30^s
Decl. „ $+50°$ 15′ „ $+51°$ 14′ } 1855.

This gives as its centre :—

R.A. 4^h 26^m 0^s. Decl. $+50°$ 44′·5 (1855).

It was observed again on January 25. The blotting out of the stars was very marked, and the object seemed more remarkable than ever. It is elliptical, major axis P=336°. It was also observed on February 16 and February 17. An attempt was made to photograph the region on January 24, but the night was very unsteady, and though the exposure was carried on for two hours, the plate only gives stars to the 12th magnitude. N.G.C. 1624, which had been picked up independently, had left

a trace on the plate, but there was no trace of the new object. A photo was again attempted on February 17, but at the end of twenty-five minutes it clouded up. Meantime I had written to Mr. Heath at Edinburgh, and he informs me that Dr. Halm found it on February 17, with the 6 in. refractor, without much difficulty. Mr. Heath also saw it, and says : "To both of us it conveyed the impression of an attenuated cloud-like object or haze, producing a difference of colour from that of the neighbouring sky, and, as you remark, dimming the tiny stars which appear to shine through it."

Ephemeris for Physical Observations of the Moon, 1898 *April* 16 *to* 1899 *January* 1. By A. C. D. Crommelin.

This ephemeris has been constructed in the same manner as those communicated by Mr. Marth, and commences at the point where his last one terminates (vol. lvii. 8, p. 613). The inclination of the Moon's equator to the ecliptic has been taken as 1°·523, the value employed by him for the last two years. As this value is used in the Libration Tables of the *Connaissance des Temps*, these tables have been utilised. But the Moon's longitude, latitude, mean longitude, and all other quantities required, have been taken from the *Nautical Almanac*. The principal term of the physical libration has also been applied, the co-efficient being 0°·037 (Franz). The co-longitude of the Sun is 90° (or 450°)—his selenographical longitude. I have considered it better to make the ephemeris continuous without any break at new Moon, as this makes it easier to follow the character of the libration curves.

Greenwich Noon.	Selenographical Colong. of the Sun.	Lat.	Geocentric Libration Sel. Long. of the Earth.	Lat.	Combined Amount.	Direction.
1898.	°		°		°	°
Apr. 16	211·79	+1·51	+4·82	−4·32	6·46	228·1
17	224·01	1·51	4·96	5·34	7·32	222·9
18	236·23	1·50	4·93	6·05	7·80	219·2
19	248·46	1·50	4·72	6·45	8·00	216·2
20	260·69	1·49	4·35	6·51	7·81	213·7
21	272·92	1·49	3·69	6·26	7·26	210·5
22	285·15	1·49	2·85	5·72	6·41	206·5
23	297·38	1·48	1·82	4·92	5·26	200·3
24	309·60	1·48	+0·61	3·92	3·96	188·9
25	321·83	1·48	−0·74	2·75	2·86	164·9
26	334·05	1·47	2·15	1·46	2·60	124·2
27	346·26	+1·47	−3·55	−0·11	3·55	91·8

Greenwich Noon.		Selenographical Colong. of the Sun.	Selenographical Lat. of the Sun.	Geocentric Libration Sel. Long. of the Earth.	Geocentric Libration Lat. of the Earth.	Combined Amount.	Direction.
1898.		°	°	°	°	°	°
Apr.	28	358·47	+1·46	−4·85	+1·26	5·00	75·4
	29	10·67	1·46	5·95	2·59	6·49	66·5
	30	22·88	1·45	6·76	3·83	7·77	60·5
May	1	35·07	1·45	7·20	4·92	8·71	55·7
	2	47·26	1·44	7·19	5·79	9·20	51·2
	3	59·44	1·43	6·70	6·36	9·25	46·5
	4	71·62	1·42	5·75	6·58	8·75	41·2
	5	83·80	1·41	4·40	6·38	7·72	34·6
	6	95·97	1·39	2·77	5·76	6·39	25·7
	7	108·14	1·38	−0·99	4·74	4·83	11·8
	8	120·32	1·37	+0·78	3·40	3·50	347·1
	9	132·50	1·35	2·39	1·83	3·04	307·4
	10	144·69	+1·33	3·76	+0·16	3·76	272·4
	11	156·88	1·32	4·83	−1·48	5·07	253·0
	12	169·09	1·30	5·59	3·00	6·32	241·8
	13	181·30	1·28	6·04	4·31	7·43	234·5
	14	193·52	1·27	6·22	5·36	8·21	229·3
	15	205·75	1·25	6·14	6·10	8·66	225·2
	16	217·98	1·23	5·84	6·52	8·74	221·9
	17	230·21	1·22	5·33	6·62	8·47	218·8
	18	242·45	1·20	4·62	6·39	7·86	215·9
	19	254·69	1·19	3·75	5·87	6·99	212·6
	20	266·94	1·17	2·69	5·10	5·76	207·8
	21	279·18	1·16	1·49	4·10	4·35	199·9
	22	291·42	+1·14	+0·17	2·93	2·93	183·3
	23	303·66	1·13	−1·23	1·64	2·05	143·1
	24	315·90	1·11	2·67	−0·28	2·70	96·0
	25	328·13	1·10	4·06	+1·10	4·22	74·8
	26	340·36	1·08	5·35	2·45	5·88	65·4
	27	352·59	1·07	6·45	3·70	7·42	60·2
	28	4·81	1·05	7·28	4·82	8·74	56·5
	29	17·02	1·03	7·73	5·73	9·59	53·5
	30	29·23	1·01	7·77	6·37	10·02	50·7
	31	41·44	0·99	7·34	6·69	9·91	47·6
June	1	53·63	0·97	6·42	6·62	9·20	44·1
	2	65·82	+0·95	−5·06	+6·13	7·97	39·5

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		°	°	°	°	°	°
June	3	78·01	+0·92	−3·34	+5·23	6·22	32·6
	4	90·19	0·90	−1·42	3·94	4·18	19·8
	5	102·38	0·87	+0·58	2·38	2·45	346·3
	6	114·56	0·85	2·47	+0·65	2·54	284·7
	7	126·75	0·82	4·13	−1·10	4·25	255·1
	8	138·95	0·79	5·47	2·75	6·13	243·3
	9	151·15	0·77	6·44	4·18	7·66	237·0
	10	163·36	0·74	7·03	5·32	8·79	232·9
	11	175·58	0·72	7·24	6·14	9·48	229·7
	12	187·81	0·69	7·11	6·62	9·74	227·0
	13	200·04	0·67	6·67	6·76	9·46	224·6
	14	212·27	0·64	5·98	6·57	8·87	222·3
	15	224·52	+0·62	5·06	6·09	7·92	219·7
	16	236·76	0·60	3·97	5·34	6·67	216·6
	17	249·01	0·58	2·74	4·36	5·14	212·1
	18	261·26	0·56	1·41	3·20	3·49	203·8
	19	273·51	0·53	+0·01	1·91	1·91	180·3
	20	285·75	0·51	−1·41	−0·53	1·51	110·6
	21	298·00	+0·49	2·81	+0·87	2·95	72·8
	22	310·25	0·47	4·14	2·24	4·72	61·6
	23	322·49	0·45	5·35	3·53	6·42	56·6
	24	334·73	0·42	6·37	4·68	7·90	53·7
	25	346·97	0·40	7·14	5·63	9·07	51·7
	26	359·20	0·37	7·59	6·34	9·87	50·1
	27	11·42	0·35	7·67	6·73	10·20	48·7
	28	23·64	0·32	7·34	6·78	9·98	47·3
	29	35·85	0·30	6·58	6·44	9·21	45·6
	30	48·05	0·27	5·39	5·70	7·87	43·4
July	1	60·25	0·24	3·83	4·56	5·97	40·0
	2	72·44	0·21	2·00	3·09	3·68	32·9
	3	84·63	+0·18	−0·04	+1·39	1·39	1·6
	4	96·81	0·15	+1·93	−0·41	1·97	258·0
	5	109·00	0·12	3·74	2·17	4·34	239·9
	6	121·19	0·09	5·29	3·75	6·51	234·7
	7	133·39	0·06	6·47	5·04	8·22	232·1
	8	145·60	+0·03	+7·23	−6·00	9·40	230·3

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1898.	°	°	°	°	°	°
July 9	157·81	0·00	+7·57	−6·59	10·03	229·0
10	170·03	−0·03	7·49	6·82	10·11	227·7
11	182·25	0·06	7·05	6·70	9·73	226·5
12	194·48	0·08	6·28	6·28	8·85	225·0
13	206·71	0·11	5·26	5·56	7·67	223·4
14	218·95	0·13	4·05	4·62	6·14	221·3
15	231·20	−0·16	2·72	3·50	4·44	217·8
16	243·44	0·18	+1·31	2·22	2·58	210·5
17	255·69	0·21	−0·11	−0·85	0·86	172·6
18	267·94	0·23	1·49	+0·55	1·59	69·7
19	280·19	0·25	2·80	1·94	3·42	55·3
20	292·44	0·27	3·98	3·26	5·13	50·7
21	304·69	0·30	5·00	4·45	6·70	48·3
22	316·93	0·32	5·84	5·45	8·00	47·0
23	329·17	0·34	6·44	6·20	8·95	46·1
24	341·41	0·37	6·78	6·66	9·49	45·5
25	353·64	0·39	6·82	6·80	9·62	45·1
26	5·86	0·42	6·54	6·56	9·25	44·9
27	18·08	−0·45	5·92	5·95	8·39	44·9
28	30·29	0·47	4·97	4·97	7·01	45·0
29	42·49	0·50	3·70	3·66	5·22	45·3
30	54·69	0·53	2·17	2·08	3·02	46·2
31	66·88	0·56	−0·46	+0·34	0·57	53·5
Aug. 1	79 06	0·59	+1·33	−1·43	1·96	222·9
2	91·24	0·62	3·06	3·10	4·34	224·6
3	103·42	0·65	4·62	4·53	6·47	225·6
4	115·61	0·67	5·88	5·64	8·17	226·2
5	127·81	0·70	6·75	6·38	9·31	226·6
6	140·01	0·72	7·20	6·72	9·86	227·0
7	152·21	0·75	7·21	6·70	9·88	227·1
8	164·42	−0·77	6·80	6 35	9·32	227·0
9	176·63	0·79	6·03	5·70	8·32	226·6
10	188·85	0 82	4·99	4·81	6·94	226·0
11	201·08	0·84	3·74	3·73	5·27	225·1
12	213·31	0·86	2·36	2·50	3·42	223·3
13	225·54	−0·87	+0·94	−1·16	1·50	219·0

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1898.	°	°	°	°	°	°
Aug. 14	237·78	−0·89	−0·46	+0·23	0·52	63·4
15	250·02	0·91	1·76	1·62	2·39	47·4
16	262·26	0·93	2·91	2·95	4·13	44·6
17	274·50	0·95	3·88	4·16	5·70	43·0
18	286·74	0·97	4·64	5·19	6·95	41·8
19	298·99	0·98	5·18	5·99	7·91	40·9
20	311·22	−1·00	5·49	6·50	8·51	40·2
21	323·46	1·02	5·57	6·68	8·68	39·8
22	335·69	1·04	5·43	6·51	8·46	39·8
23	347·92	1·06	5·07	5·98	7·83	40·3
24	0·13	1·08	4·49	5·10	6·78	41·3
25	12·34	1·10	3·70	3·91	5·40	43·4
26	24·54	−1·12	2·70	2·46	3·65	47·7
27	36·74	1·15	1·52	+0·83	1·73	61·4
28	48·92	1·17	−0·18	−0·86	0·88	168·1
29	61·10	1·19	+1·25	2·51	2·81	206·5
30	73·28	1·21	2·68	3·99	4·79	213·9
31	85·45	1·23	4·02	5·19	6·59	217·8
Sept. 1	97·63	1·25	5·14	6·05	7·93	220·4
2	109·80	1·26	5·95	6·52	8·80	222·4
3	121·98	1·28	6·38	6·61	9·19	224·0
4	134·17	1·30	6·40	6·34	9·02	225·3
5	146·36	1·31	6·02	5·76	8·31	226·3
6	158·55	1·32	5·27	4·92	7·22	227·0
7	170·75	−1·33	4·22	3·88	5·74	227·4
8	182·95	1·34	2·96	2·68	4·00	227·9
9	195·16	1·35	1·58	1·37	2·09	229·1
10	207·38	1·36	+0·16	−0·01	0·16	266·4
11	219·59	1·37	−1·19	+1·35	1·80	41·4
12	231·81	1·38	2·40	2·67	3·60	42·0
13	244·04	1·39	3·40	3·89	5·17	41·2
14	256·27	1·40	4·14	4·94	6·47	40·0
15	268·50	1·41	4·61	5·77	7·39	38·6
16	280·73	1·41	4·80	6·33	7·98	37·2
17	292·96	1·42	4·72	6·56	8·07	35·8
18	305·19	−1·43	−4·42	+6·43	7·78	34·5

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1898.	°	°	°	°	°	°
Sept. 19	317·41	−1·44	−3·95	+5·94	7·13	33·6
20	329·63	1·45	3·33	5·10	6·12	33·2
21	341·84	1·46	2·62	3·95	4·74	33·5
22	354·05	1·47	1·82	2·56	3·15	35·4
23	6·25	1·48	0·96	+1·00	1·39	43·8
24	18·43	1·49	−0·02	−0·62	0·62	178·1
25	30·61	1·50	+0·98	2·21	2·41	203·9
26	42·79	1·51	2·01	3·67	4·18	208·7
27	54·96	1·51	3·03	4·89	5·77	211·8
28	67·12	1·52	3·97	5·80	7·02	214·4
29	79·28	1·52	4·76	6·35	7·94	216·9
30	91·44	1·53	5·31	6·53	8·42	219·1
Oct. 1	103·60	−1·53	5·55	6·34	8·43	221·2
2	115·77	1·53	5·45	5·82	7·97	223·1
3	127·93	1·53	4·99	5·02	7·08	224·8
4	140·10	1·53	4·19	4·00	5·78	226·3
5	152·28	1·53	3·14	2·81	4·21	228·2
6	164·46	1·53	1·87	1·52	2·41	230·9
7	176·64	1·52	+0·48	−0·18	0·51	249·4
8	188·83	1·52	−0·92	+1·18	1·50	38·2
9	201·03	1·52	2·26	2·49	3·36	42·2
10	213·22	1·51	3·41	3·70	5·03	42·7
11	225·43	1·51	4·32	4·77	6·44	42·2
12	237·64	1·50	4·91	5·63	7·49	41·1
13	249·85	−1·50	5·14	6·23	8·10	39·5
14	262·06	1·49	5·03	6·52	8·22	37·7
15	274·28	1·49	4·58	6·45	7·93	35·4
16	286·49	1·48	3·87	6·00	7·14	32·8
17	298·71	1·48	2·96	5·18	5·96	29·7
18	310·92	1·48	1·95	4·04	4·48	25·8
19	323·13	1·47	−0·92	2·64	2·80	19·2
20	335·32	1·47	+0·10	+1·07	1·07	354·7
21	347·51	1·47	1·06	−0·56	1·20	242·1
22	359·70	1·46	1·96	2·14	2·91	222·5
23	11·87	1·46	2·78	3·59	4·52	217·7
24	24·04	−1·45	+3·51	−4·81	5·96	216·1

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	°	°	°	°	°	°
Oct. 25	36·20	−1·44	+4·15	−5·74	7·06	215·9
26	48·35	1·43	4·68	6·34	7·86	216·4
27	60·51	1·42	5·04	6·56	8·27	217·5
28	72·65	1·41	5·20	6·43	8·29	219·0
29	84·80	1·40	5·11	5·96	7·85	220·6
30	96·94	1·39	4·75	5·20	7·04	222·4
31	109·09	−1·37	4·11	4·19	5·87	224·5
Nov. 1	121·24	1·36	3·20	3·01	4·38	226·7
2	133·39	1·34	2·07	1·71	2·69	230·4
3	145·55	1·32	+0·76	−0·35	0·84	245·3
4	157·71	1·31	−0·64	+1·02	1·20	32·1
5	169·88	1·29	2·06	2·35	3·13	41·3
6	182·05	1·27	3·39	3·58	4·94	43·4
7	194·22	1·26	4·55	4·67	6·49	44·2
8	206·40	1·24	5·45	5·57	7·80	44·4
9	218·59	1·22	6·00	6·23	8·66	43·9
10	230·78	1·20	6·16	6·59	9·03	43·1
11	242·97	1·19	5·90	6·61	8·86	41·8
12	255·17	−1·17	5·23	6·24	8·17	40·0
13	267·37	1·15	4·21	5·49	6·92	37·5
14	279·58	1·14	2·92	4·38	5·26	33·7
15	291·78	1·12	1·49	2·97	3·33	26·7
16	303·98	1·11	−0·02	+1·35	1·35	0·8
17	316·17	1·09	+1·40	−0·35	1·44	256·0
18	328·36	1·07	2·66	2·02	3·35	232·8
19	340·54	1·06	3·75	3·43	5·06	227·6
20	352·71	1·04	4·62	4·81	6·69	223·9
21	4·87	1·02	5·27	5·79	7·82	222·3
22	17·03	1·00	5·70	6·42	8·60	221·6
23	29·18	0·98	5·90	6·69	8·90	221·4
24	41·33	−0·96	5·89	6·60	8·84	221·7
25	53·47	0·93	5·65	6·18	8·34	222·4
26	65·60	0·91	5·18	5·46	7·53	223·5
27	77·74	0·88	4·50	4·48	6·34	225·1
28	89·88	0·86	3·61	3·31	4·91	227·5
29	102·01	−0·83	+2·53	−2·00	3·21	231·7

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1898.	°	°	°	°	°	°
Nov. 30	114·15	−0·80	+1·29	−0·62	1·43	244·3
Dec. 1	126·29	0·77	−0·07	+0·79	0·79	5·1
2	138·43	0·75	1·49	2·15	2·62	34·7
3	150·58	0·72	2·91	3·42	4·48	40·4
4	162·73	0·69	4·24	4·55	6·23	43·0
5	174·88	0·67	5·42	5·49	7·69	44·6
6	187·04	−0·64	6·35	6·21	8·89	45·7
7	199·20	0·61	6·97	6·65	9·62	46·3
8	211·38	0·58	7·19	6·78	9·85	46·7
9	223·55	0·56	6·99	6·55	9·58	46·9
10	235·73	0·54	6·33	5·94	8·67	46·8
11	247·92	0·51	5·25	4·94	7·19	46·7
12	260·12	0·49	3·80	3·61	5·24	46·5
13	272·31	0·46	2·10	2·00	2·90	46·4
14	284·50	0·44	−0·27	+0·24	0·36	48·4
15	296·70	0·42	+1·55	−1·54	2·19	225·2
16	308·89	0·39	3·24	3·20	4·57	225·3
17	321·07	0·37	4·69	4·61	6·57	225·5
18	333·24	−0·35	5·82	5·71	8·15	225 6
19	345·41	0·32	6·61	6·44	9·25	225·7
20	357·57	0·29	7·04	6·78	9·79	226·1
21	9·73	0·27	7·12	6·75	9·83	226·6
22	21·87	0·24	6·88	6·38	9·36	227·2
23	34·01	0·20	6·35	5·71	8·57	228·0
24	46·15	0·17	5·59	4·77	7·38	229·5
25	58·29	0·14	4·62	3·63	5·87	231·8
26	70·42	0·11	3·48	2·34	4·18	236·1
27	82·55	0·08	2·22	−0·96	2·42	246·6
28	94·68	0·05	+0·87	+0·45	0·98	297·4
29	106·81	−0·02	−0·53	1·84	1·91	16·1
30	118 94	+0·02	1·94	3·15	3·69	31·6
31	131·08	+0·05	3·30	4·32	5·44	37·6
1899.						
Jan. 1	143·21	+0·08	−4·58	+5·32	7·02	40·7

7 *Vanbrugh Park Road, Blackheath, S.E.:*
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